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THE TREATMENT OF DIABETES MELLITUS.

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MR. CHAIRMAN AND GENTLEMEN:—It would not be possible, within the limits to which this paper is necessarily restricted, to discuss satisfactorily the pathology or even the clinical history of diabetes mellitus, although the disease in question is one of the most interesting, as well as obscure affections which the physician is called upon to treat. While the study of diabetes and its attendant disorders of general nutrition presents difficulties, as regards questions of causation and pathology, that seem almost insurmountable, when attention is once directed to the simple problem of the presence of sugar in the urine, this condition is now easily and certainly recognizable. It is probably true that sugar exists in the urine of a certain number of persons, unattended with symptoms, so that it is detected only by accident or may never be revealed, such persons having no apparent occasion to seek medical advice. In an experience in life insurance examinations extending through a period of nearly thirteen years, I have found a small quantity of sugar in the urine of applicants who supposed themselves to be perfectly healthy; but within the time mentioned, only five such cases have come under my observation. Three of these applicants are now living and are presumably in good health, the sugar in the urine having been noted from eight to twelve years ago; one case was lost sight of, and one applicant is reported to have died of hæmoptysis nine months after the examination of the urine. During the time mentioned; viz., twelve years and nine months, I examined 1884 persons who supposed themselves to be in good health, and nearly always made examinations of the urine. All of the applicants, with one or two exceptions, were males. The proportion, therefore, of apparently healthy persons in whose urine I have found sugar is very small (1 in 377); but even this shows that sugar may be present in the urine, either as a transient or an insignificant condition or existing without any of the general symptoms of diabetes.

In the great proportion of cases of diabetes that come under observation, attention is directed to the condition of the urine by certain general symptoms; such as excessive thirst, persistent polyuria, a sensation of dryness of the mouth and fauces, fatigue after moderate muscular exertion, or some slight affection of the external genitals. In a case of diabetes that I have had under treatment for nearly four years, now under observation, the patient first consulted a physician for herpes progenitalis, which led to an examination of the urine. In females, persistent pruritus of the vulva is often the first circumstance pointing to the possible existence of diabetes. In several cases, I have detected sugar in the urine when pruritus vulvæ was the only trouble complained of by patients. So constant is this symptom, that diabetes should always be suspected when the pruritus persists without any apparent cause and resists ordinary measures of treatment. The pruritus is seldom absent when the proportion of sugar in the urine is considerable.

Detection of Sugar in the Urine.—As far as purely clinical examination of the urine is concerned, the great desideratum is a simple test, easy and rapid in its application, upon which one can rely with absolute confidence. I shall pass over, without discussion or even mention, the different tests employed for the detection of sugar, except the one known as Fehling's. When the Fehling's liquid is properly prepared and carefully used, there can be no error in the results. If a quantity of this test, however, be made and kept for some time, it is liable to change so as to become more or less unreliable. This want of stability in the test-liquid has long been recognized by those accustomed to urinary examinations; and a few years ago I prepared three separate liquids, which I mixed in certain proportions for use as required. Even this did not prove to be entirely satisfactory. Within the last year, two separate liquids have been prepared by Dr. E. R. Squibb, and are kept by him for sale, in which form, the test seems to leave nothing to be desired in the qualities of accuracy and ease of application. The test, as it is now prepared by Dr. Squibb, is simply perfect; but so much depends upon its proper use, that I shall venture to detain the "Section" with an account of its application and the necessary precautions to be adopted. These precautions are simple and demand no special skill; but they often become very important, especially in determining with certainty the absence of sugar.

The two test-liquids are prepared by Dr. Squibb according to the following formulæ:

For the Solution of Cupric Sulphate.—Use purified sulphate of copper, in granular crystals, air-dried. Weigh 277 grains (17.32 grammes) of the salt and dissolve it in about 4 fluidounces (120 c.c.) of distilled water, adding about 4 minims (¼ c.c.) of pure sulphuric acid. Add distilled water to this solution to make 8½ fluidounces. (260 c.c.)

For the Solution of Alkaline Tartrates.—Weigh 2 ounces, 391 grains (87.5 grammes) of re-crystallized sodio-potassic tartrate, or Rochelle salt, and dissolve it in about 6 fluidounces (175 c.c.) of distilled water. Filter the solution, if necessary, and add to it a clear solution of 386 grains (25 grammes) of caustic soda in about 1¾ fluidounces (50 c.c.) of distilled water. Add distilled water to this solution to make 8½ fluidounces (260 c.c.)

These two solutions are to be kept in separate bottles for use. If they be made with accuracy and mixed together in equal proportions, 200 grains of the mixture will be decolorized by exactly one grain of sugar, or each cubic centimetre of the mixture will be decolorized by 0.005 of a gramme of sugar. The liquids can therefore be employed for quantitative estimates, although I shall describe the use of the test simply for determining the fact of the presence or absence of sugar.

For use in qualitative analysis, the two liquids may be roughly mixed in about equal proportions in a test-tube, or they may be measured accurately and diluted with about an equal volume of distilled water. The latter process should be resorted to in all delicate analyses.

For ordinary use the following process may be employed:

Mix in a test-tube equal volumes of the two liquids so that the mixture will extend in the tube to the length of about an inch.

Bring the mixture to the boiling point and then add to the boiling test a quantity of urine equal to that of the test.

Bring the mixture of the test-liquid and urine to the boiling point and then allow it to cool.

If no distinct and opaque reddish or yellowish precipitate be present when the mixture of test and urine has become cool after the second boiling, it is absolutely certain that no sugar is present.

All these precautions are essential; and I have repeatedly examined specimens of urine in which the characteristic precipitate due to the presence of sugar did not occur until one or two minutes had elapsed after the second boiling.

In very delicate testing, take a definite quantity of the copper-solution, add an equal quantity of distilled water, add then of the solution of alkaline tartrates a quantity equal to the quantity of the copper-solution, and add finally distilled water in the same quantity. When this mixture is boiled, if the test be not absolutely perfect, there will be a precipitate before the urine is added. The mixture, if perfect, may be used in the same way as the simple undiluted mixture of the two solutions.

When sugar is present in the urine, an opaque yellowish or reddish precipitate appears at some time during the process, the promptness of its appearance and its quantity being in direct proportion to the quantity of sugar.

It is often important to be able to determine, at least approximatively, the quantity of sugar discharged in twenty-four hours or its proportion per fluidounce. Using the volumetric process, this estimate requires some practice and occupies from twenty to thirty minutes; but the "differential density method" recommended by Roberts, is very simple and is sufficiently accurate for ordinary purposes. With a little practice, indeed, it may be employed by intelligent patients.

Two specimens of diabetic urine are taken, about four ounces of each, one for comparison and the other for analysis. To one is added a lump of German yeast, about the size of a filbert, in a bottle with a cork nicked to allow the escape of gas; and the other specimen is placed in a similar bottle tightly corked. The bottles are then put aside in a warm place, as the mantel-piece in winter or in the sun in summer. In the course of twenty-four hours, fermentation will have been completed in the specimen to which yeast has been added. If the specific gravity of the two specimens be then compared, the fermented specimen will be found much the lighter, from loss of the sugar which has been decomposed into alcohol and carbonic acid. The difference in the density of the two specimens, expressed in degrees of the urinometer, will represent the number of grains of sugar per fluidounce in the urine. For example, if the specific gravity of the fermented specimen be 1010, and the specific gravity of the unfermented specimen, 1040, the urine contains thirty grains of sugar per fluidounce. In this process, it is essential to compare the density of the two specimens at the same temperature. If German yeast cannot be obtained readily, about a teaspoonful of ordinary baker's or brewer's yeast may be used.

Relations of the Specific Gravity of Urine to the Proportion of Sugar.—It has long been recognized that the specific gravity of the urine bears no definite and constant relation to the proportion of sugar in cases of diabetes. In a case that came under my observation in December, 1883 and has been under treatment until the time of writing (April, 1884) on Dec. 29, 1883, the specific gravity was 1038, with 28.4 grains of sugar per fluidounce. The next day, the specific gravity was 1036 and the proportion of sugar was nine grains per fluidounce. In another very interesting case now under treatment, I found four grains of sugar per fluidounce, the urine having a specific gravity of only 1011½. These remarkable variations in the specific gravity, occurring without any relation to the quantity of sugar, are generally dependent upon the proportion of urea, the absolute quantity of which is often very largely increased in cases of diabetes. I have often found crystals of uric acid as a persistent condition in diabetic urine, sometimes associated with a deposit of oxalate of lime.

The time allotted to me does not admit of a discussion of the possible relations of the nutritive conditions connected with diabetes to the excessive elimination of urea or the frequent presence of crystals of uric acid; but it is very important to remem-

ber that urine of a comparatively low specific gravity may contain sugar. Within a week, in another case in which the urine is examined every three or four days, I found a marked sugar-reaction in a specimen of urine with a specific gravity of 1010. I have also repeatedly found sugar in urine of a specific gravity of about 1020, the quantity of urine in twenty-four hours being normal. The fact, then, that the quantity and specific gravity of the urine are normal does not in itself exclude sugar; although, in most cases of diabetes, the quantity of urine is increased and its specific gravity is notably high. In a case of diabetes very elaborately reported by Pavy, sugar was found in the urine when the specific gravity of the specimens was, on different occasions, 1010, 1011, 1012, and 1013.[1] In cases in which diabetes is suspected, the physician is not justified in excluding the disease when he finds no increase in the quantity of urine and a normal specific gravity; and the facts just mentioned show that, in all cases of this kind, the urine should be carefully tested for sugar.

What constitutes Diabetes Mellitus?—A patient with abnormal thirst, dryness of the mouth, suffering from fatigue following slight muscular exertion, progressively losing strength and weight, and passing an abnormally large quantity of urine of high specific gravity and containing sugar, has the disease known as diabetes mellitus; but the various symptoms just enumerated may exist in greater or less degree or some of them may be absent. In addition to these symptoms, others may exist; such as, abnormal dryness of the skin, deficient perspiration on exercise or in warm weather, pruritus of the vulva, a tendency to furuncles, unusual liability to "take cold," reduction in the general temperature of the body, an excessive appetite, failure of the generative functions, etc., but these are not necessarily present in cases of diabetes.

On the other hand, none of the general symptoms that I have mentioned may be observed; the urine may be normal as regards quantity and specific gravity; but still sugar may constantly exist in small quantity. In such instances, which are not very infrequently observed, the constant, necessary, and invariable symptom of diabetes is present; namely, glycosuria. Strictly speaking, perhaps, patients with no general symptoms, with no increase in the quantity of urine, and with urine of normal specific gravity, may be said to be affected with glycosuria, but not to have diabetes. In the great majority of cases, however, unless the glycosuria be transient and dependent upon some recognizable or temporary cause, certain of the general symptoms of diabetes will sooner or later become developed, unless the glycosuria be removed by treatment. Still, even without treatment, persons may live in what seems to be perfect health for years, constantly passing considerable quantities of sugar. I can now call to mind three cases of this kind; and several cases, in which I have found sugar in the urine without any other diabetic symptoms, have passed from under my observation.

I shall have little to say concerning the etiology and pathology of diabetes. The brilliant physiological experiments, which began with the discovery of the sugar-producing function of the liver by Claude Bernard, in 1848, have failed, in a great measure, to fulfil the expectation that they would lead to a definite comprehension of the pathology of diabetes. I believe it to be true that the liver is a sugar-producing organ. The experiments of Pavy, in which he showed that the liver-substance does not actually contain sugar during life were, in my opinion, harmonized with those of Bernard, by experiments made by me in 1869.[1] In these experiments, I found no sugar in an extract of the liver taken from a living dog and put into boiling water in ten seconds, while sugar was present in blood taken from the hepatic veins. I am convinced that the liver is constantly forming sugar during life, but that this sugar, as fast as it is produced, is washed out of the sugar-producing organ by the blood-current. Experiments have shown, also, that the sugar contained in the food, as well as that resulting from the digestion of starch, is normally destroyed in the organism. That the sugar-forming function of the liver may become exaggerated beyond the power of the organism to destroy the excess thus produced was demonstrated by the remarkable experiments of Bernard, in which he induced temporary diabetes in animals by mechanical irritation of the floor of the fourth ventricle, by stimulating the pneumogastric nerves, or by introducing irritating vapors into the lungs; but, although cases of traumatic diabetes occur in the human subject, they are exceedingly rare. No such case has as yet come under my observation.

I do not propose, at this time at least, to offer any theory with regard to the causation or pathology of diabetes, the cause of death in the so-called diabetic coma, or the supposed development in certain cases of acetonæmia. The discussion of these points has, up to the present time, been very unsatisfactory. We all know that patients presenting, in a well-marked degree, certain characteristic symptoms, in addition to glycosuria, are affected with a very grave disease, the pathology of which is imperfectly understood. The sugar resulting from digestion is in great part discharged in the urine. The nutritive processes are seriously disturbed. The power of resistance to other diseases is impaired. What is remarkable and quite interesting, in its relations to our ideas of the production of animal heat, the failure to consume the hydrocarbons seriously affects the power of resistance to cold, and the general temperature of the body is habitually 95° or 96° Fahr., instead of about 98½°. This latter point I state upon the authority of many writers; and in a case now under treatment, the temperature in the axilla has constantly been about 96½°. As the patient improved, the temperature was increased to a fraction over 97°, but it has not yet reached the normal standard.

Being brought, then, face to face with a disease, very obscure in its pathology, and not infrequent in its occurrence, the practical question, to which I intended to devote the main part of this paper, is, how

[1] Pavy, *Nature and Treatment of Diabetes*, London, 1869, p. 288, *et seq.*

[1] Flint, Jr. *Experiments undertaken for the Purpose of reconciling some of the Discordant Observations upon the Glycogenic Function of the Liver.—New York Medical Journal*, 1869, vol. viii, p. 373, *et seq.*

far it is amenable to treatment. To this question I shall devote what remains of the time at my disposal.

Treatment.—In a course of lectures by Cantani, delivered at the clinical hospital of the University of Naples, in the spring of 1872, there occurs the following statement, italicised by the author:

"*Diabetes has become to-day a disease easily and certainly curable, provided that the treatment* (*cure*) *be not begun too late.*"[1]

The cases which Cantani details in support of this rather startling statement show certainly the most remarkable effects of treatment. Judging from the account of these cases, the general proposition that diabetes is a disease, in the main easily and certainly curable, is not too decided and absolute. Since I have been engaged in treating cases of this disease, my experience, though not extending over many years, has led me to the conviction that the claim made by Cantani is not extravagant.

In the great majority of cases in which patients will submit to certain measures of treatment as soon as it is established that they are suffering from diabetes, or as soon as glycosuria is recognized, it is possible to effect either a cure of the disease or a removal of most of the characteristic symptoms, with the exception, perhaps, of the occasional appearance of a small quantity of sugar in the urine.

Time does not permit me to discuss fully the treatment recommended by different writers. Cantani relies mainly upon dietetic measures, although he attaches considerable importance to the exhibition of lactic acid and the alkaline lactates. Of course the treatment by eliminating sugar and starch from the diet is by no means novel. Dating from the time of Rollo, it has had the earnest support of Bouchardat, Pavy, Seegen, and many others. I desire to state at the outset, that the main and almost the sole reliance of the physician should be upon diet; and that the suppression of starch and sugar should be practically absolute. Bearing this fact constantly in mind, in considering the different measures of treatment, I shall divide them into dietetic, general, and medicinal.

Dietetic Treatment.—In 1869, a patient was sent to me from Omaha, Neb., whom I found to be suffering from many of the distressing symptoms of diabetes.

On Nov. 20, 1869, he passed 224 fluidounces of urine in the 24 hours, with a specific gravity of 1,035. The quantity of sugar passed in the 24 hours was 18 ounces and 30 grains, and the quantity of urea was 624 grains. I recommended a diet-table by no means as rigid as the one I now employ, and he left for home. For several years I heard from this patient, either personally or through his physician in Omaha, from time to time, and he was reported as apparently well, but occasionally passing a small quantity of sugar. He continued the diet more or less faithfully for two or three years, but took a little bread. About five years after, I was accosted in the street by this patient, who reported himself as feeling perfectly well and giving but little attention to his diet. At this time I did not have an opportunity of examining the urine. The patient has since died, and I heard from his widow that this occurred in August, 1881, his death being immediately due to inflammation of the bowels after a few days' illness. "The diabetes was much improved and troubled him very little."

This case, during the time when I was constantly receiving favorable reports, seemed to me to be quite remarkable; and in 1880, having frequent occasion to recommend a diet for diabetics, I carefully compiled an anti-diabetic diet-table, which I have since used constantly in cases that have come under my observation, and which I shall present as an appendix to this paper. In preparing this table, my object has been to secure a diet sufficiently nutritious but free from starch and sugar, using as a basis the admirable list given by Bouchardat;[1] and I have endeavored to adapt the articles and their preparation to the customs of our own country, adding to it, when possible, in order to secure the greatest available variety of food. Selecting, however, every dish known in the culinary art, without reference to the trouble or expense of its preparation, a rigid diet is by no means easy of enforcement. Patients at first have an intense craving for bread; and this desire is so nearly universal that almost all writers on diabetes suggest some substitute for this important article of food. I do not hesitate to say, however, without specifying any one of the so-called anti-diabetic breads and flours as especially bad, that all the articles of this kind in our markets are absolutely unreliable and most of them fraudulent. I have analyzed, or caused to be analyzed, nearly all of the so-called bran-flours, and gluten-flours, and have invariably found large quantities of starch. Two specimens said to be free from starch, which were analyzed with great care by a competent chemist, were found to contain a greater proportion of this principle than exists in ordinary wheat-flour. Most of the so-called diabetic breads are pasty, heavy, and become extremely distasteful. A patient now under occasional observation, having procured a new bread which was so agreeable to the taste that he took it freely and with relish, imagined that he had found at last an article which would be regarded by diabetics as the greatest boon. This bread was made of flour which contained about 80 per cent. of starch.[2] The effects of this fraud upon the patient were quite serious. His health had become nearly restored and the sugar had disappeared from the urine. Under the use of the bread, the sugar returned, and it was several weeks before it disappeared again under a strict diet. In the rigid dietetic treatment, bread should be absolutely interdicted, or, in case patients should refuse to submit to a strict diet, a small quantity of crust of bread taken with an abundance of butter may be allowed under protest.[3]

A rigid diet, without bread, should be continued

[1] CANTANI, *Le diabète sucré et son traitement diététique*, Paris, 1876, p. 386.

[1] BOUCHARDAT, *De la glycosurie ou diabète sucré*, Paris, 1875, p. clxxxvi.

[2] Ordinary wheaten flour contains about 70 per cent. of starchy matters.

[3] Since this article has been in print, I have seen a bread made by George V. Hecker & Co., 205 Cherry street, New York City, made of wheaten flour said to contain only 5 per cent. of starch. This bread is light and agreeable, but it requires great care in its preparation. A specimen of the bread was examined by Prof. G. A. Doremus, who found a little more than one per cent. of starch.

until the sugar has disappeared from the urine and all the diabetic symptoms have been removed. Although many diabetics rebel under this regimen, and the execution of this measure demands, on their part, much self-denial and fortitude, patients may be encouraged to persevere, by the statement that the craving for saccharine and starchy articles is likely to diminish and may almost disappear after a few weeks. I have now under observation and treatment several patients who have actually lost all desire for most of the interdicted articles of food.

In cases in which the treatment is followed by an apparent cure, sugar disappearing from the urine, a gradual return to the normal diet should be begun about two months after the glycosuria has ceased; but it is of the greatest importance, during this part of the treatment, to keep patients, if possible, under constant observation, examining the urine at least once in five or six days. When the sugar disappears, patients may regard themselves as permanently cured, and no general symptoms present themselves for some time after glycosuria has returned under a mixed diet. Several unfortunate examples of this have come under my observation.

General Treatment.—Measures of general treatment are to be directed mainly to promoting the proper action of the skin, which is often harsh and abnormally dry, and to general muscular exercise. Systematic rubbing, as practiced by massage, and Turkish or Russian baths once a week, if they be not contraindicated by some complicating conditions, are useful. A reasonable restriction in the taking of liquids is quite important in diminishing the quantity of urine. Under the dietetic treatment the excessive thirst is almost always relieved; but when this persists, it may often be temporarily met, as far as dryness of the mouth is concerned, by taking small pieces of ice from time to time instead of drinking water. I do not know that any reliance is to be placed upon the use of the various mineral waters that are said to exert a curative influence over the disease in question. Alcoholic stimulants are to be avoided. I have seen several cases of diabetes in which the disease seemed to be attributable to the abuse of alcohol, especially the habitual and excessive drinking of champagne. In certain cases, some kind of alcoholic beverage seems to be necessary to maintain the vital powers. For this purpose, a fairly good, sound claret has seemed to me to be the best form in which alcohol may be taken. Spirits should be interdicted or given very sparingly, and not more than a pint of claret should be taken daily.

Patients suffering from diabetes lose, to a certain extent, their capacity for sustained mental effort. They should be cautioned, therefore, against excessive intellectual work. Mental anxiety and "worry" over business or other affairs exert a very unfavorable influence on the progress of the disease. In some cases apparently cured, I have noted a return of the glycosuria, which seemed to be fairly attributable to mental causes. The insomnia rarely demands the use of narcotics and is usually relieved with the other symptoms by the anti-diabetic diet.

The various minor complications that are liable to occur can usually be overcome by appropriate treatment. The occurrence of boils is very common and they are likely to be persistent and annoying. When the tendency to boils is very marked, the sulphide of calcium is useful, although this agent does not seem to exert a curative influence over the diabetes itself. The sulphide of calcium has been recommended very highly as a remedy controlling the glycosuria; but it is often disagreeable to patients and disturbs digestion. In a few instances in which I have employed it for a considerable period, it has not seemed to affect the discharge of sugar, and I regard it as useful only to combat the furuncular tendency. It is dangerous to rely upon drugs to any extent in the treatment of this disease. Patients willingly put faith in remedies rather than in a rigid diet; but, after all, diet is the main and almost the only reliance in treatment.

A very important, and perhaps the most important, measure of general treatment is systematic muscular exercise, not carried to the extent of producing excessive fatigue. This may be taken in the form of gymnastics, or of out-door exercise, such as riding or athletic sports; but patients should always be cautioned to avoid "taking cold." If a patient suffering from diabetes can be made to develop his muscular strength by moderate and systematic exercise, not too prolonged, and followed by a proper and not excessive sense of fatigue and some perspiration, with a good reaction after bathing and rubbing, much will be gained in the way of treatment. This is strongly recommended by all writers upon diabetes.

The diminished power of resistance to cold which exists nearly always in diabetics renders it necessary to enjoin great care in avoiding exposure to the vicissitudes of the weather, and the constant protection of the body by warm clothing, especially flannels next the skin.

Medicinal Treatment. There is no remedy that exerts a curative influence over diabetes in the absence of proper dietetic measures. Opium, the bromides, sulphide of calcium, various mineral waters, and other medicinal agents that have been recommended from time to time, have all proved very unsatisfactory in practice. Of course it is difficult to estimate the value of drugs in this as in many other diseases, particularly as the physician is not justified, in my opinion, in neglecting to enforce a rigid diet which, in itself, in the great majority of cases, exerts a most decided influence over the glycosuria and the general symptoms. On theoretical grounds, Cantani recommends lactic acid, taken in the form of a "lemonade," in small quantities throughout the day. The formula for this mixture is the following:

Pure lactic acid	℥iss to ℥v.
Aromatic water	℥v to ℥j.
Water	Ojj.

This remedy is regarded by Cantani as useful in many cases but not essential. I have little experience in its employment.

Keeping in mind the small reliance to be placed in the efficacy of drugs unconjoined with dietetic measures, I must bear testimony to the apparent advantage to be derived from the use of the arsenite of

bromine, recently proposed by Clemens. While I have not felt justified in using this remedy to the exclusion of the anti-diabetic diet in treatment, for the reason that the bad effects of an unrestricted diet frequently persist for some time, I have noted very marked effects from Clemens's solution in controlling the discharge of sugar and some of the distressing symptoms, particularly the excessive thirst; so that, aside from simple measures to relieve sleeplessness, constipation, or other intercurrent difficulties, I have lately been in the habit of prescribing, in addition to the diet, three drops of Clemens's solution, three times daily, in a wine-glass of water, after each meal, gradually increasing the dose to five drops. The following is the formula for this remedy, which I have had prepared by Mr. William Neergaard, 1183 Broadway, New York City, and which may be written for under the name of "Clemens's Solution of Arsenite of Bromine."

"Liquor brom-arsen consists simply of a chemical union of arsenious acid and bromine, dissolved in water and glycerine, in such a manner that two drops represent the twenty-fourth part of a grain of arsenite of bromine."[1]

In a case of diabetes of more than five years' standing, now under treatment, the patient has been taking Clemens's solution constantly, with the exception of a single week, from Dec. 27, 1882, to April 2, 1884, more than fifteen months, without any unpleasant effects. I began with a dose of two drops, three times daily, gradually increased to five drops. On May 13, 1883, the urine having been free from sugar, with the exception of a trace on two or three occasions, for thirteen weeks, I stopped the arsenite of bromine for one week, the anti-diabetic diet being continued. At the end of the week, sugar was found in large quantity in the urine. The use of the arsenite of bromine was then resumed. At the end of the first week, the sugar still existed in small quantity. At the end of the second week, the sugar had disappeared and there was no return of glycosuria for six weeks. The patient then left the city and committed many indiscretions in diet. Seven weeks later, I examined a specimen of urine and found it loaded with sugar, with a specific gravity of 1,030. While absent from New York, the patient had indulged in peas, egg-plant, stuffed tomatoes, green corn, ice-cream, charlotte russe, peaches, raspberries, blackberries, and melons. On September 20, after returning to New York and resuming a strict diet with the exception of the crust of half a white roll three times daily, the patient improved. The urine, on September 20 had a specific gravity of 1031 and was loaded with sugar. The following week the sugar was much diminished in quantity, and it disappeared at the end of the second week.

Summary of Treatment.—The more I study the cases of diabetes that have come under my observation, especially those that are now under treatment, in connection with the writings of those who have faithfully followed the dietetic plan, notably Bouchardat and Cantani, the more thoroughly am I convinced that the prognosis in a recent and uncomplicated case of this disease in an adult is invariably favorable, provided, always, that the proper measures of treatment be rigidly enforced. In the hope of convincing the profession that this statement is reliable, I shall, at the risk of what may appear to be needless repetition, give a summary of treatment, with brief statements of the progress of cases that I am now actually observing.

At the outset, patients should be impressed with the fact that they are suffering from a grave disorder, and that everything depends upon their full coöperation in the treatment, which treatment is essentially dietetic. The diet-table should be carefully studied, and the diet regulated and carried out absolutely.

In case a rigid anti-diabetic diet does not promptly influence the glycosuria, it may be well to subject a patient to an absolute fast for twenty-four hours and follow this with the anti-diabetic regimen. This rather harsh measure is suggested by Cantani. I shall not hesitate to employ it in cases in which it may seem to be required, although no such case has as yet come under my observation.

The various measures that I have mentioned under the head of *General Treatment* should be enforced, especially systematic daily muscular exercise. A moderate system of training on the plan adopted by athletes is most useful; and this, if continued, will do much to render a cure permanent after a return to the normal diet.

The return to a normal diet should be gradual, and during this time the urine should be frequently examined, the rigid diet being resumed at the first reappearance of sugar in the urine; but all alcoholic excesses, the immoderate use of sweet fruits, and any use of sugar, should be interdicted at all times. A patient who has once had diabetes is always liable to a return of the disorder. He must lead a thoroughly careful, hygienic, and temperate life. In the words of Bouchardat, "you will not be cured except on the condition that you never believe yourself to be cured."[1]

While I believe that the physician is justified in encouraging patients to expect relief, and even cure, in recent, uncomplicated cases, the diet is all important, and its regulation cannot be expected to be perfect without professional aid in its enforcement. A diabetic is never safe from a return of his disease, even when he believes himself to be cured; and under no circumstances should he pass more than a few weeks without an examination of the urine.

The arsenite of bromine, or Clemens's solution, appears to be useful. We may begin with 3 drops three times daily in a little water immediately after eating, gradually increasing the dose to 5 drops. This may be continued for weeks and months without producing any unfavorable effects; but the administration of this remedy does not supply the place of the dietetic treatment, which should be enforced in all cases. A rigid diet should be continued for two months, at least, even in the mildest cases of the disease. It may be necessary, in certain cases, to continue it for a longer period, even twelve or more months.

There is probably no such disease as intermittent

[1] *Medical Times*, Philadelphia, Dec. 2, 1882, p. 160.

[1] BOUCHARDAT, *De la Glycosurie ou Diabète Sucré*. Paris, 1875, p. 49.

diabetes. In some instances glycosuria occurs during the season of sweet fruits, when they are indulged in excessively, and disappears when the diet is changed; but these are mild cases of diabetes, excluding those in which a transient glycosuria follows the inhalation of irritating vapors, the taking of anæsthetics, etc.

Robust or corpulent persons are more tolerant of the disease than those who are feeble or spare, and the glycosuria yields, in such cases, more readily to treatment.

Diabetes occurs at all ages. Bouchardat mentions a case in an infant of 3 years, although the disease is rare before the age of 12. The most unfavorable cases are those which occur before the age of puberty. An adult male presents the most favorable conditions for cure. In old persons, when the disease is of long standing, the dietetic treatment will secure practical immunity from nearly all the distressing symptoms, although the glycosuria may not be entirely removed.

A study of any of the diet-papers recommended will make it evident that those who are able to follow the required regimen, without regard to the cost of articles of food, present much more favorable conditions, as regards the prospect of cure, than persons in straightened or indigent circumstances. Diabetes, however, occurs in all classes and is by no means a rare disease. A hospital devoted to such cases, where the dietetic treatment could be strictly carried out, would be a boon to the rich and poor alike.

Cases.—I have accounts, more or less complete, of fifty cases of diabetes. A certain number of these cases have been lost sight of; others were followed out in their histories to a fatal termination; and twelve, exclusive of a few that are reported to be cured, are still under either observation or treatment.

Of these fifty cases, sixteen have been lost sight of, nineteen are either known to be living or are under observation, and twelve have died at periods between nine months and over twelve years after I first examined the urine.

Of the seven patients who are living, but whom I do not consider as under observation, one passes sugar constantly and is under an imperfect anti-diabetic diet, but is in what may be called fair health; two are reported as cured, although I have not examined the urine for a long time; four I simply know to be living.

The twelve cases that are under observation are instructive as indicating the value and influence of treatment.

Case A.—The patient, a gentleman thirty-eight years of age, first became aware that he suffered from diabetes mellitus about June 1, 1883. He is five feet five inches in height, and weighs one hundred and twenty-nine pounds. A year ago he weighed one hundred and sixty pounds. He suffered from excessive discharge of urine, with increased appetite, thirst, dryness of the mouth, sleeplessness, fatigue on slight exercise, and, indeed, most of the symptoms of diabetes; but a careful physical examination failed to reveal any other disease. At the time I first saw him, he had been taking quinine and various tonic remedies and had been subjected to an imperfect anti-diabetic diet. At this time, December 29, 1883, he passed eighty ounces of urine in twenty-four hours, containing in all 3,072 grains of sugar. He was immediately put upon a strict diet, taking no bread, drinking very little, and relieving the thirst temporarily by taking pieces of ice. In addition, he took three drops of Clemens's solution three times daily, and continued to take ten grains of quinine each day. After forty-eight hours of this treatment, his intense thirst and excessive urination disappeared, but he expressed himself as feeling rather weak although generally much better. The effect, however, upon the discharge of sugar was remarkable. He passed, during the second twenty-four hours of treatment, forty-three ounces of urine, and the total quantity of sugar was reduced from 3,072 grains to 387 grains.

I heard from this patient January 19, 1884, and received a specimen of the urine of the twenty-four hours of January 17th. For the twenty days since December 29, 1883, he had maintained an absolute anti-diabetic diet, taking no bread. During this time, he took three drops of Clemens's solution three times daily. He had gained three-quarters of a pound in weight. He had suffered somewhat from indigestion but was otherwise quite well. "The very large appetite and thirst are very materially lessened." The quantity of urine in twenty-four hours was forty-eight and one-half fluid-ounces; specific gravity, 1026; absolutely no sugar; there was rather an abundant deposit of amorphus urates with a number of crystals of uric acid. As far as the diabetic condition is concerned, the general symptoms had disappeared as well as the sugar in the urine.

On January 25, 1884, the dose of Clemens's solution was increased to five drops three times daily. The urine was free from sugar. There was no sugar in the urine on January 27 and 29. He was then allowed the crust of half a French roll at breakfast.

On February 4, 1884, I saw the patient again. He had been at home and had committed some slight indiscretions in diet. The urine had a specific gravity of 1030 and contained a small quantity of sugar. The strict diet was resumed.

On February 10, 1884, there was a trace of sugar in the urine.

On February 24, 1884, the patient still under a strict diet and the use of Clemens's solution, there was no sugar in the urine. The patient went home, feeling perfectly well, and promised to send a specimen of urine in two weeks. At no time since the beginning of treatment was there any excessive quantity of urine.[1]

Case B.—The patient is a gentleman fifty-seven years of age, five feet eleven and one-half inches in height, weighing one hundred and twenty-two pounds. He had suffered from diabetes to his knowledge for about one year, with thirst, fatigue after moderate exertion, and other mild symptoms. He had been under a moderate anti-diabetic diet for some weeks. After he came under my observation, his urine, under a strict anti-diabetic diet, was either entirely free from

[1] On April 29, 1884, I received a specimen of urine from this patient. The quantity in twenty-four hours was said to be about fifty fluidounces. The specific gravity was 1022½ and it contained a trace of sugar. The general health was reported as perfect.

sugar or contained merely a trace, for ten months. He had no symptoms and regarded himself as cured. For about three months he took four drops of Clemens's solution three times daily.

On January 17, 1884, he presented himself, passing a large quantity of urine of a specific gravity of 1,027, and loaded with sugar. Having regarded himself as permanently cured, he had returned to his old diet, including sugar, and had stopped the arsenite of bromine for six months. He felt perfectly well but had noticed for somedays that he was passing a large quantity of urine. He was again put upon an anti-diabetic diet (which I fear is not strictly followed) with six drops of Clemens's solution twice daily. On February 7, 1884, he passed a normal quantity of urine of a specific gravity of 1,022, containing but a trace of sugar.

In this case, I cannot secure a strict adherence to the diet and regular examinations of the urine.[1]

Case C.—This patient has been under observation since October, 1880. He was at that time 53 years of age, five feet eight and one-half inches in height, and weighed from 168 to 172 pounds. The diabetes had been recognized a few weeks before he came under my observation, and he had been subjected to an imperfect anti-diabetic diet. He was immediately put upon to a strict diet, and from October 21, 1880, to May 18, 1881, his urine generally contained no sugar, although there was occasionally a trace. In this case the diet was strictly followed, and the patient soon lost his desire for prohibited articles, even bread.

On May 18, 1881, he was allowed the fruits in season, to be taken without sugar. On June 27 he was allowed a little bread. His urine was practically free from sugar until February 17, 1882, with the exception of an occasion on November 5, 1882, when it had a specific gravity of 1,029 and contained considerable sugar following a slight excess at table in taking claret and whiskey and water.

On February 17, 1882, his urine had a specific gravity of 1,026 and contained considerable sugar. He had been living rather freely for some time without committing any actual excesses at table. He moderated his living and was given, in addition to the strict diet, one-quarter of a grain of sulphide of calcium three times daily. From February 17, 1882, to September 1, 1883, his urine was practically free from sugar when examined on ten different occasions, once, only, presenting a mere trace. During the entire treatment he has taken considerable exercise in walking. He took the sulphide of calcium rather irregularly for six months, but it was very disagreeable.

On January 11, 1883, he began to take the arsenite of bromine, which he continued rather irregularly.

On January 23, 1884, his weight had increased to 175½ pounds. Since September 1, 1883, his diet had been practically unrestricted. His urine had a specific gravity of 1021 and contained a small quantity of sugar. He was put on a moderate anti-diabetic diet and the dose of arsenite of bromine was increased to five drops. On February 7, 1884, the sugar was still marked in the urine, but he indulged rather too freely in claret at dinner and drank some brandy and soda during the evening. From February 7, to April 3, 1884, the urine had been nearly always free from sugar.

This may almost be called a case of cure. For the greatest part of the time from October, 1880, to April, 1884, three and one-half years, the urine has been practically free from sugar, for some of the time under an ordinary diet. During this period, sugar has appeared temporarily and in small quantity, possibly as a consequence of occasional indiscretions in the use of wine, which could not by any means be regarded as excesses in a person in ordinary health.[1]

Case D.—This is the case of a lady, rather stout, fifty-nine years of age, who came to me for treatment in December, 1882. The patient has already been referred to in connection with the fact of the existence of sugar in urine of a low specific gravity, (1,011½) and the return of glycosuria immediately following the suspension for one week of the administration of arsenite of bromine.

In December, 1880, the patient was in a deplorable condition, suffering from some of the most distressing symptoms of diabetes. She suffered intensely from thirst, night and day, and was forced to pass the urine nearly every hour. She also suffered greatly from pruritus vulvæ. Her disease was of five years' standing, and she had been subjected to various forms of treatment, but never to a strict diet. She had consulted many distinguished physicians in this country and in Europe.

On December 16, 1882, she passed 128 ounces of urine, of a specific gravity of 1,c36, containing twenty-two grains of sugar per fluidounce, or 2,816 grains in the twenty-four hours. The next day she was put upon a strict anti-diabetic diet.

On December 22, 1882, the daily quantity of urine was reduced to 52 ounces, with a specific gravity of 1026, containing eight grains of sugar per fluidounce, or 416 grains in the twenty-four hours. The urine constantly presented crystals of uric acid. The thirst, pruritus, and constant desire to pass urine were relieved.

With the exception of one week, this patient took Clemens's solution, two drops three times daily, the dose finally increased to five drops, from December 27, 1882, to April 2, 1884. The treatment during this period consisted of the diet and Clemens's solution, with occasional remedies to act upon the bowels. She has been almost constantly under treatment, and I made 91 examinations of the urine up to April 6, 1884. Her urine is now examined regularly once a week.

Under treatment, the quantity of sugar in the urine diminished until the glycosuria disappeared January 27, 1883, about thirty days after the first examination. From January 27, 1883, to April 6, 1884, with the exception of about six weeks passed at a watering-place in the summer of 1883, under very unfavorable

[1] I saw this patient on April 29, 1884, and he reported himself as perfectly well, but I did not have an opportunity of examining the urine.

[1] On April 28, 1884, this patient reported himself as perfectly well. His urine had a specific gravity of 1,020½ and contained no sugar. The diet had been not absolutely strict but was what may be called moderately anti-diabetic.

conditions as regards diet, the urine has either been free from sugar or has contained a very small quantity. The quantity of urine has been constantly normal, and the general diabetic symptoms have never reappeared. She now uses the anti-diabetic diet with the crust of one-half of a French roll at each meal, a pint of cream daily, and a little fruit in season.

While this cannot be called an instance of perfect cure, the fact that the patient lives comfortably and in apparently good health under a diet that is not particularly irksome shows that cases of long standing and presenting very unfavorable features are by no means hopeless. This case presented to a remarkable degree the example of a loss of desire for prohibited articles of food. She now looks forward to eating melons in season, which is about the only strong wish she has expressed for food not suited to her condition.[1]

Case E.—The patient in this case is a gentleman about fifty years of age, living in Ohio. I examined his urine May 24, 1878, and November 17, 1881, for some reason not connected with a suspicion of diabetes and found no sugar. On May 4, 1882, I again examined the urine, on account of certain diabetic symptoms, and found a large quantity of sugar. He was at once put on the anti-diabetic diet, which he attempted to carry out by himself at his home in Ohio. In January, 1884, he reported that all his symptoms had been relieved and that he suffers nothing unless he commits indiscretions in diet.

Case F.—The patient in this case is a gentleman about fifty years of age, and of medium muscular and adipose development. Having been suffering for some months from diabetic symptoms, his urine was examined by me on March 22, 1878. I then found a specific gravity of 1022 and a large quantity of sugar. He was at once put upon a moderate anti-diabetic diet.

On April 26, 1878, I found the urine normal, and the diabetic symptoms had disappeared. Between April 26, 1878, and January 10, 1882, I examined the urine seven times, always finding it normal.

On October 14, 1882, he passed ninety-six ounces of urine in the twenty-four hours, with a specific gravity of 1027 and containing 4 grains of sugar per fluidounce. His diet for some time had been irregular, and he had depended on various remedies, such as the bromides and the sulphide of calcium. He then began to take the arsenite of bromine, but his diet, though moderately anti-diabetic, was still imperfectly regulated. His urine, examined February 20, May 18, and May 28, 1883, contained a small quantity of sugar. On August 15, 1883, I examined the urine and found a trace of sugar.

This patient suffers very little from diabetic symptoms. I have little doubt that the glycosuria could be arrested by a few weeks of strict dietetic treatment.

Case G.—The patient is a lady, rather stout, and about seventy-five years of age. Attention was directed to the urine on February 7, 1884, by excessive thirst and urination, with pruritus vulvæ. Before I examined the urine, it was reported to me that she was passing it in large quantity, the specific gravity being 1040, and that it was loaded with sugar. Under an anti-diabetic diet and the arsenite of bromine, in three days the quantity of urine was reduced to the normal standard and the diabetic symptoms disappeared. I examined the urine on February 13, 19, 28, March 4, 11, 17, 21, 25, 31, and April 5, 1884. The urine, with one exception, presented sugar in small but variable proportions, but its quantity was usually normal, and the specific gravity varied between 1007 and 1020. The urine on one occasion, with a specific gravity of 1010, contained a trace of sugar. On March 17, the urine had a specific gravity of 1007 and contained no sugar. The general diabetic symptoms are now entirely relieved. The only fault in the diet is that the patient takes a quart of milk daily. The progress of this case is quite favorable up to the present time.[2]

Case H.—The patient is a large and rather corpulent man, about 60 years of age. I examined the urine December 27, 1882, and found it with a specific gravity of 1027 and containing a considerable quantity of sugar. He was at once put upon the anti-diabetic diet. Under this treatment the glycosuria and other diabetic symptoms disappeared. In July, 1883, he was attacked with hemiplegia, from which he has substantially recovered. He was reported in March, 1884, as perfectly well, having returned to the normal diet.

Cases I, J, K and L.—These are cases of patients who are constantly passing sugar in large quantities, under little or no treatment, but who enjoy fair health. In one of these cases the patient obstinately refuses to regulate the diet, and although he suffers but little from diabetic symptoms, he has become greatly reduced in weight and strength within the past two years. A young daughter of this patient, whom I saw repeatedly and who never followed out the anti-diabetic diet, died of diabetes about three years ago. Another patient has fair health under a rather irregular diet. He is so situated as to be unable to carry out a strict regimen. The two other patients are large and corpulent men, who pass immense quantities of sugar, with no restriction in diet or in drinking.

Of the fifteen cases of death the reports are generally imperfect. Four are reported as having died of diabetes; one, of diabetic coma, possibly acetonæmia; three, of albuminuria; one, of apoplexy; one, of hæmoptysis; one, of "inflammation of the bowels"; and in the remaining four cases I have not been able to learn the cause of death.

Case M.—I have already referred to this case. The patient was a stout, heavy man, about 40 years of age. I examined him November 20, 1869. He then passed 224 ounces of urine in the twenty-four hours,

[1] The urine of this patient is examined regularly once a week, and there has been no sugar, with the exception of a trace on one occasion, for twelve weeks. The last examination was made on May 4, 1884, and no sugar was found. With the exception of sugar, the diet has been but little restricted for three weeks. For the last three weeks the patient has been taking about a pint daily of the lactic acid drink recommended by Cantani.

[1] From April 5 to May 2, 1884, I have made six examinations of the urine. The specific gravity has been between 1012½ and 1020 and a small quantity of sugar has always been noted, but there have been no general diabetic symptoms. The diet has not been rigidly carried out.

[2] This patient died of apoplexy, June 1, 1884.

with a specific gravity of 1035, containing 18 ounces and 30 grains of sugar, and 624 grains of urea. He was immediately put upon a moderate anti-diabetic diet and returned to his home in Nebraska. I heard from time to time for several years that he enjoyed good health and had little or no glycosuria unless he committed serious indiscretions in diet. He died of "inflammation of the bowels," after a short illness, in August, 1881, nearly twelve years after I first saw him, in 1869.

Case N.—On November 6, 1879, I saw in consultation a lady about 58 years of age, rather spare in figure, who had been suffering for some months with diabetes. At this time the quantity of urine was not notably increased, the specific gravity was 1030, and it contained a small quantity of sugar. The treatment had consisted mainly of an imperfect anti-diabetic diet. A more rigid diet was recommended, but it was not strictly enforced. On November 10 and 15, 1877, the urine contained a trace of sugar. On November 29, 1877, the urine was free from sugar, and the patient was much improved. She left for her home in Cuba, and I saw her again on Sept. 2, 1880, when the urine was still free from sugar. In July, 1881, she was passing large quantities of sugar, and I learned that for several months the diet had been unrestricted and she had eaten sweets and fruits immoderately. She returned to the anti-diabetic diet, and I found the urine free from sugar with all the diabetic symptoms relieved on August 2, 1881. She then returned to her old habits of eating, and the urine was found loaded with sugar on Sept. 27, and Oct. 29, 1881. I learned that she died "of diabetes," never having returned to the anti-diabetic diet, in Cuba, in 1882. During the last few weeks of her life she was much prostrated, suffering intensely with boils and carbuncles, which were probably the immediate cause of death.

The diet-table which follows is adapted to those who are able to provide themselves with any kind of food required without regard to cost rather than to persons of restricted pecuniary resources; but I have recognized the fact that those who are subjected to the anti-diabetic diet should secure every possible variety of food. In making this table, I have drawn largely from those already published, particularly the list of permissible articles given by Bouchardat, but, after many trials of the so-called anti-diabetic flours and bread, I have come to the conclusion, as I have already stated, that they are nearly all unreliable. I prefer to make patients abstain entirely from bread, or I allow the crust of half a French roll two or three times daily if I cannot eliminate bread altogether from the diet. The gluten-breads, so-called, are not only unreliable, but they soon become very distasteful. When ordinary bread is allowed, the physician knows, at least, about how much starch is taken.

APPENDIX.

DIET-TABLE.

BREAKFAST.—Oysters stewed, without milk or flour; clams stewed, without milk or flour.

Beefsteak, beefsteak with fried onions, broiled chicken, mutton or lamb chops, kidneys, broiled, stewed, or deviled; tripe, pig's feet, game, ham, bacon, deviled turkey or chicken, sausage, corned-beef hash without potato, minced beef, turkey, chicken, or game, with poached eggs.

All kinds of fish, fish-roe, fish-balls, without potato.

Eggs cooked in any way except with flour or sugar, scrambled eggs with chipped smoked beef, picked salt cod-fish with eggs, omelets plain or with ham, with smoked beef, kidneys, asparagus-points, fine herbs, parsley, truffles, or mushrooms.

Radishes, cucumbers, water-cresses, butter, pot-cheese.

Tea or coffee, with a little cream and no sugar. (Glycerine may be used instead of sugar if desired.)

Light red wine for those who are in the habit of taking wine at breakfast.

LUNCH OR TEA.—Oysters or clams cooked in any way except with flour and milk, chicken, lobster, or any kind of salad except potato, fish of all kinds, chops, steaks, ham, tongue, eggs, crabs, or any kind of meat, head-cheese.

Red wine, dry sherry, or Bass's ale.

DINNER.—Raw oysters, raw clams.

Soups.—*Consommé* of beef, of veal, of chicken, or of turtle, *consommé* with asparagus-points, *consommé* with okra, ox-tail, turtle, terrapin, oyster or clam, without flour or milk; chowder, without milk or potatoes, mock turtle, mullagatawny, tomato, gumbo *filet.*

Fish, etc.—All kinds of fish, lobsters, oysters, clams, terrapin, shrimps, crawfish, hard-shell crabs, soft-shell crabs. (No sauces containing flour or milk.)

Relishes.—Pickles, radishes, celery, sardines, anchovies, olives.

Meats.—All kinds of meat cooked in any way except with flour, all kinds of poultry without dressings containing bread or flour, calf's head, kidneys, sweet-breads, lamb-fries, ham, tongue, all kinds of game; veal, fowl, sweet-breads, etc., with currie but not thickened with flour. (*No liver.*)

Vegetables.—Truffles, lettuce, romaine, chiccory, endive, cucumbers, spinach, sorrel, beet-tops, cauliflower, cabbage, Brussels-sprouts, dandelions, tomatoes, radishes, oyster-plant, celery, onions, string-beans, water-cresses, asparagus, *artichauts,* Jerusalem artichokes, parsley, mushrooms, all kinds of herbs.

Substitutes for Sweets.—Peaches preserved in brandy without sugar, wine-jelly without sugar, *gelée au kirsch* without sugar, *omelette au rhum* without sugar, *omelette à la vanille* without sugar, *gelée au rhum* without sugar, *gelée au café* without sugar.

Miscellaneous.—Butter, cheese of all kinds, eggs cooked in all ways except with flour or sugar, sauces without sugar, milk, or flour.

Almonds, hazel-nuts, walnuts, cocoanuts.

Tea or coffee with a little cream and without sugar. (Glycerine may be used instead of sugar if desired.)

Moderately palatable ice-creams and wine-jellies may be made, sweetened with pure glycerine; but although these may be quite satisfactory for a time they soon become distasteful.

Alcoholic Beverages.—Claret, burgundy, dry sherry, Bass's ale or bitter beer. (No sweet wines.)

PROHIBITED.

Ordinary bread, cake, etc., made with flour, sugar, desserts made with flour or sugar, vegetables, except those mentioned above, sweet fruits.

SILICATE OF SODIUM BANDAGE.[1]

BY EUGENE O. BARDWELL, M.D., MOLINE, ILL.

GENTLEMEN:—I have frequently conversed with physicians of ability, age and experience, who have never employed and consequently never appreciated the value of silicate of sodium bandage as a permanent dressing in fractures of the long-bones.

Silicate of sodium is a solution of silica and sodium carbonate, containing 20 parts of the former to 10 parts of the latter; it is of a syrupy consistence with a specific gravity of from 1,300 to 1,400; it is also called soluble glass or water glass; it possesses many advantages over starch, dextrine, or plaster of paris, in that it is more easily applied, is more uniform in thickness, is more cleanly, and last but not by any means least, it does not contract on drying as do the others. The mode of application is very simple and easy, a common roller bandage is first applied, the joints and bony prominences being protected by cotton; this is followed by another roller which is painted in *situ* with the solution by means of an ordinary flat varnish brush. Other rollers are applied and painted in the same manner until four or five thicknesses of bandage coated with the silicate are in position. This is usually sufficient, but if thought necessary, strips of the roller may be laid longitudinally between the bandages and coated likewise with the solution. Pieces of tin or zinc may be placed between the folds if thought expedient, but I have never required their aid, the bandage without them being very hard and stiff when dry, and effectually preventing the slightest movement of the fracture. The silicate commences to harden at once and is usually hard enough to cut in 24 hours, and if properly put on will keep the fracture in position from the time of application. In fracture of the arm, leg or thigh, the bandage should of course commence at the fingers or toes to avoid possible strangulation; with this precaution, any one who is fit to apply a roller at all, can use this preparation with perfect safety and without fear.

At the end of one day it is usually advisable to cut the roller, as it will probably not fit the part closely enough, as the equable pressure of an unyielding substance from the extremity to a point above the fracture will generally quiet muscular action, and consequently, in removing a prominent source of irritation, reduce the swelling of the part affected. No one would think of using an application of this nature until the swelling and engorgement following the injury had perceptibly diminished. It is not, however, either necessary or desirable to wait until the swelling has entirely disappeared.

The bandage should usually be cut at the end of twenty-four hours. This may be done with Seuten's pliers, which are sold by dealers in surgical instruments. They are heavy shears with the blades curving upward, the nether blade being flattened to facilitate its introduction under the bandage. In lieu of these a narrow strip of tin or zinc may be placed along the limb under the first silicated layer, and with this for a director any strong cutting instrument may be utilized. Commencing at the extremity, the bandage is cut through from one end to the other, usually on the external aspect of the limb, for the sake of convenience. At distances which are variable, owing to circumstances, pieces of a roller or of tape are then passed around the bandage, or splint, as it is now more properly called, and tied firmly enough to hold the injured member in proper position. If the limb is smaller than when the bandage was applied, a longitudinal section is cut out wide enough to leave a small space between the edges of the splint when tightened, so that should it become looser in the absence of the surgeon it may be easily tightened with the tapes by the patient himself or any one in the household. In cases of compound fracture, a trap may be cut anywhere for purposes of drainage, examination or treatment. As a rule, we apply this dressing about the seventh day following the injury, and in ordinary cases the patient is able to be out of bed two or three days later, able to sit on a chair and get around on crutches with perfect comfort, so far as pain is concerned, and without any prejudicial effect on the union of the fracture. We are aware that a certain length of time must inevitably elapse after fracture before complete union can take place. The general health and physical condition of the patient will, however, as it is good or bad, shorten or lengthen this period perceptibly; and certainly the sooner the patient can get out of bed with perfect comfort and safety the sooner will his general condition approximate the normal. I think that the great majority of cases of fracture of the long bones, either simple or compound, can, with perfect safety to themselves and with gratification to their friends and their physician, get about the house in from ten to twelve days.

A few cases may be adduced in illustration. In the fall of 1879 I was sent for to see a man who had been injured by the fall of a building. I found his right tibia fractured at the lower third and his right ankle badly sprained, the fracture was reduced and the usual splints applied, it was six weeks before he could get around the house, and several months before he entirely recovered. About two months later W. H. while working in a saw mill, was caught by the carriage, thrown down and injured. On examination I found his right ankle severely sprained and the tibia of the corresponding side fractured at the

[1] A paper read before the Rock Island County Medical Society at Rock Island, Ill., April 1, 1884. Published by request of the Society.

lower third, with an open wound leading to the fracture where the patient said the bolt entered which broke his leg; the fracture was reduced, the wound covered with a compress saturated with carbolized oil (1 to 40) and the limb kept in position by sand bags. At the end of six days, the swelling having materially diminished, I applied the silicated roller, cutting a trap over the wound, the next day I cut the bandage and put on the tapes. Three days later W. H. was out on crutches, said he had no pain, and the splint was left off entirely at the end of four weeks. These two cases under the same treatment would in all probability have followed the same course, although the latter, being compound, was the most serious.

In the spring of 1882 I was sent for to see a boy of 8 years, who had been injured by the fall of a pile of lumber. He had sustained a fracture of the right tibia at the middle third, the silicated bandage was applied the fifth day. I cut it the next day and showed the mother how to adjust the tapes to keep the splint tight enough, the boy got up that afternoon and I did not see him again until he walked into my office with his father five weeks later. In August, 1883, G. C., aged 50 years, working in a saw mill, received a blow from the end of a heavy plank which fractured the right femur at the junction of the middle with the lower third. I reduced the fracture and used extension to the amount of twelve pounds for four days, when it was reduced to eight pounds; the sixth day the silicated bandage was put on from the toes to the trochanters, the weights were left on until the roller became hard and were then removed, the splint opened, and tapes adjusted; careful measurement showed the leg to be the same length after the weights were removed as before.

The patient got about on crutches without pain or uneasiness at once, and at the end of two weeks walked on crutches one and one-half miles from his house to the mill, and returned the same afternoon without any bad results whatever; when I last saw him in October, he walked without a cane and without a limp. I have treated numerous other cases with this preparation, as have other physicians of my acquaintance, and the results have been uniformly successful and gratifying to all concerned. It is my belief that the great majority of cases of fracture of the leg and thigh should be out of bed within two weeks from the date of the injury, and that they may be with perfect safety to the injured member and with great benefit to the physical and mental condition of the patient. Of course there are cases where complications may exist or arrise to prevent, but they will prove exceptions to the rule.

I have used the silicate in place of plaster of paris in the application of Sayre's jacket for spinal curvature, and believe it much to be preferred to the latter in every way. In conclusion I would request the members of this society who have not used this dressing to give it a trial before passing judgment. Those who have used it will, I am sure, endorse my statements.

MEDICAL PROGRESS.

MEDICINE.

M. PASTEUR ON THE VIRUS OF RABIES.—This important paper, which was communicated to the Paris Académié des Sciences, and which is exciting so much comment at present, is literally translated (*Medical Times*) as follows:

The important fact that certain infective poisons vary in respect to their virulence, and that insusceptibility to a virus may be secured by means of another virus of less intensity, is at the present moment not only an abstract scientific discovery, but has found an application in the domain of practice. Research being once turned in this direction, it is easy to understand how great an interest attaches to investigations into the methods of attenuation appropriate to new viruses. I have the honor, on the present occasion, to bring under the notice of the Academy an advance in this direction, in reference to rabies.

1. If the poison of rabies be transmitted from the dog to the monkey, and then from monkey to monkey, its virulence diminishes with each inoculation. If the virus, which has been thus enfeebled by inoculation from monkey to monkey, be then retransmitted to a dog, a rabbit, or a guinea-pig, it still remains attenuated. In other words, the virulence never returns at once to the degree found in the mad dog in the streets.

2. The virulence of the poison of rabies is increased when it is transmitted from rabbit to rabbit, or from guinea-pig to guinea-pig. When the virulence has thus increased and reached its maximum in the rabbit, the virus still retains this high degree of virulence when transmitted to the dog, and is evidently much more intensely virulent than the virus of the mad dog of the streets. Under these conditions, indeed, the poison is so virulent, that when inoculated into the circulation of a dog, fatal rabies is the invariable result.

3. Although the virulence of the poison is intensified in its passage from rabbit to rabbit, and from guinea-pig to guinea-pig, it requires many successive inoculations before it recovers its maximum virulence, when it has been previously attenuated in the monkey. Further, the poison found in the mad dog of the streets, which, as I have just said, is far from being of maximum virulence, when it is inoculated in the rabbit, requires to be passed through many individual rabbits before it attains that maximum.

If we apply rationally the results I have just communicated, we can easily render dogs proof against rabies. The investigator may have at his disposal the virus of rabies in different degrees of attenuation; the non-fatal kinds preserving the economy from the effects of the more active and fatal kinds. Let us take an example. We take the virus of rabies from a rabbit which has died after inoculation by trephining at the end of a period of incubation, exceeding by several days the shortest period of incubation commonly met with in the rabbit. This period invariably occurs between the seventh and eighth day

after inoculation by trephining with poison of maximum virulence. The virus from a rabbit, with the longest incubation period, is inoculated, again by trephining, in a second rabbit; the poison from this rabbit in a third. Each time the poison, which is becoming less and less virulent, is communicated to a dog. The latter is at length found capable of resisting a poison of fatal virulence. It becomes, in fact, entirely proof against rabies, when the poison of the mad dog of the streets is introduced into its system, either by intra-venous inoculation or by trephining.

By inoculations with the blood of animals I have been able greatly to simplify the operation of vaccination, and to produce in the dog the most marked insusceptibility to this disease. I shall shortly communicate to the Academy the total result of what I have found in connection with this point.

It will be of considerable interest, both now and up to the distant epoch when rabies has become extinct as the result of vaccination, to be able to prevent the disease developing after bites by rabid dogs. On this point the first experiments that I have made give me the greatest hopes of success. Thanks to the length of the period of incubation of rabies when communicated by the bite of a rabid animal, I have reason to believe that we can with certainty produce a condition of insusceptibility in those who are bitten, before the fatal malady is due.

The first results are very favorable to this view, but it will be necessary to repeat the experiments to an infinite extent on all manner of animals, before we shall have the courage to try this mode of prophylaxis on the human subject.

The Academy will understand that, in spite of the confidence inspired by the numerous experiments I have made during the last four years, it is not without apprehension that I now publish facts which point to nothing less than a possible prophylaxis against rabies.

If I had had sufficient material at my disposal, I should have preferred to defer this communication until I had asked some of my colleagues at this Academy and at the Academy of Medicine to examine the conclusions I have just made known. In deference to these scruples and motives, I took the liberty of writing a few days ago to Mr. Fallières, Minister of Public Instruction, asking him to be good enough to appoint a commission to whom I might submit the dogs which have been made proof against rabies. The crucial test which I would propose, would consist in the first place in taking from my kennels twenty dogs proof against rabies, and placing them side by side with twenty dogs intended to serve as my witnesses. We should then have these forty animals bitten successfully by mad dogs. If the facts which I have enumerated are correct, the twenty dogs which I believe to be proof against the disease would all remain healthy, while the twenty witness dogs would become infected with rabies. In a second and not less conclusive experiment, we should take forty dogs—twenty vaccinated before the Commission, and twenty not vaccinated. The forty dogs would then be inoculated by trephining with the virus of the mad dog of the streets. The twenty vaccinated dogs would be proof against the infection, while the other twenty would all die of rabies, with symptoms either of paralysis or madness.

CASES OF EPILEPSY, WITH UNUSUALLY SLOW PULSE.—At a recent meeting of the Harveian Society of London (*Medical Times*), Mr. St. George Mivart read the notes of a case of epileptiform convulsions in a retired policeman aged 61 years, which were of six years' standing—the pulse rate ranging now from 20 to 24 beats per minute. The convulsions were not frequent, the last being eight weeks previous. There was a slight systolic mitral murmur, together with an aortic systolic murmur. He considered the case to be one of fatty heart, with atheromatous deposit about the valves; and considered the seizures to be essentially of a syncopal character, modified by the epileptic tendency of the patient.

Dr. Hare related at some length a closely analogous case of unusually slow pulse associated with *petit-mal*. The pulse had gradually dropped to the unparalleled rate of 13½ per minute, at which it was maintained for a considerable period, ultimately rising to 30 and 40. The patient, a nobleman, aged 57, had, up to the time of his illness, displayed the greatest activity, both mental and physical. Death occurred in a fit. No abnormality of the heart sounds had been observed during life, and at the *post-mortem* examination the valves were found competent, and the heart but slightly enlarged and not fatty—with the exception of slight enlargement of the kidneys, the other organs were practically normal.

ESBACH'S METHOD FOR ESTIMATING THE QUANTITY OF ALBUMEN IN URINE.—Dr. Henry Veale gives this method at some length in the British *Medical Journal*. The method consists in measuring in a graduated tube, the height of the deposit given by a measured quantity of albuminous urine, when treated with a solution containing picric acid and citric acid in the proportions respectively of one and two per cent.

The test tube is somewhat stouter than an ordinary test tube. It is six inches long, and about nine-sixteenths of an inch in diameter. On its outer surface it bears a scale of seven degrees, diminishing in size from below upwards, whereby the quantity of albumen represented by the deposit is intended to be read off in grammes per litre, *i. e.*, in parts per 1,000. Somewhat above this scale is a dotted line marked U (urine), and at a certain distance above it another line marked R. (re-agent).

The test solution is made by dissolving one part of picric acid and two parts of pure air-dried citric acid by the aid of heat, in about 80 or 90 parts of distilled water, adding subsequently enough water to make exactly 100 parts—or 35 grains of picric acid and 70 grains of citric acid in 8 fluid ounces of distilled water.

1. The urine should be filtered.

2. If not distinctly acid, it should be rendered so by the cautious addition of acetic acid, drop by drop, until it changes litmus paper to a brick-red.

3. If the specific gravity exceed 1010, the urine

should be diluted by one or more volumes of water, so as to bring it below that figure.

4. If, after the first trial, the urine should be found to contain more than 4 per 1,000 of albumen, it should be diluted so as to bring the deposit below the line 5 of the tube; and in either of these two cases the indication given by the deposit must be doubled or trebled according to the degree of dilution.

5. With the urine thus prepared, fill the tube up to the line U. Add the re-agent up to the line R. Close the tube with an india-rubber plug, and gently turn it up and down a dozen times, so as thoroughly to mix without frothing the contents.

6. Let the tube remain at rest in the upright position for twenty-four hours, and, at the end of that time, read on the scale the amount of the deposit.

Example—A patient passes in twenty-four hours in cubic centi-metres of urine—35¼ fluid ounces of specific gravity 1024—a filtered portion of this is diluted with twice its bulk of water, and the deposit, after 24 hours, reaches to the line 3 of the tube. Then, this number multiplied by 3, on account of the dilution, indicates 9 grammes per litre (*i. e.*, 9 parts per 1,000) as the quantity of dry albumen contained in the specimen. Of course the same number divided by 10 will give the parts per hundred, and, if this be multiplied by the factor 4.4 (strictly 4.375) it will give, very nearly, the number of grains per fluid-ounce; thus, 9x4.4=3.96.

Dr. Veale finds Esbach's method to afford results that are accurate enough for all clinical purposes. It is simple enough to be performed as a daily duty by any intelligent nurse, or even by the patient himself. It is certainly better adapted for cases in which the loss of albumen is large, as in nephritis and certain cardiac diseases, than for those in which the albuminuria is slight and transitory; but, in the latter, a daily quantitative estimation of the albumen discharge would very seldom be considered requisite.

A THERMOSTATIC NURSE.—After the description of the couveuse which has been given in these pages, it will be well to refer to an English invention of somewhat the same character which is now in use in the City of London Lying-in Hospital, by Dr. Clement Godson. The apparatus, according to the *Lancet*, is made upon the same principle as the Champion Incubator for eggs, from which it differs in having a cradle above the water-tank instead of a drawer below. The case of the thermostatic nurse is of wood, and is divided horizontally into an upper and a lower compartment by a shallow enclosed tank of water, which extends to within two inches of the wood work all round, this interval being left to allow of the free access of air from the lower to the upper part. Above the water tank, and supported on slips of wood, is a cradle for the reception of the infant, which is kept in view through a glass window on the top of the apparatus. This sash is hinged at the back to a portion of the cover, about four inches wide. Affixed to this part of the cover is the lever-plate, the alarum, and the thermometer, the scale of the latter being outside and under immediate observation. Through the bottom of the apparatus a hole three inches in diameter permits the entrance of a liberal supply of pure air, which constantly traverses two layers of very coarse canvas kept continuously wet. This is obtained by a simple arrangement of a metal tray with central perforated cap fitting over the opening in the bottom, and a coarsely-perforated zinc plate, coated on its upper surface by the canvas which dips into the water all round the edges of the tray. By this means the canvas is continually absorbing the moisture, while it is receiving a regular supply of heat from the bottom of the water-tank; thus the air entering is uniformly moistened and heated. Altogether outside the apparatus there is a gas flame, the heat from which passes through a flue shaped like the letter U, so as to traverse twice the length of the water-tank, by which means the water is heated. The great advantage of this apparatus, however, is seen in the automatic arrangement which it possesses for keeping the internal temperature uniform. A hermetically-sealed metallic capsule, two inches square and containing a small quantity of a liquid which boils at 90° F., is fixed in the space at the head of the cradle; and in the center of the upper part of this capsule is a button, having a cup-shaped depression, in which rests the lower end of a stiff wire, which passes out through the top of the apparatus, where it comes into contact with a light lever, which is pivoted to a brass plate. From the free end of this lever hangs a little damper, which rests on the top of the chimney under which the gas-jet burns. As soon as the temperature in the infant's room reaches 90° F. the liquid in the capsule will boil, and the vapor thus produced converts the flat capsule into a rounded one, thereby raising the rod, which, acting on the lever, at once lifts the damper off the chimney, allowing the heat from the flame to escape by that outlet, and preventing the further heating of the water-tank. If a higher temperature than the boiling point of the liquid within the capsule be desired, this can be obtained by moving a weight along the lever toward the end to which the damper is attached; the further it is placed in this direction, the greater is the heat produced. The alarm is made to ring whenever the internal temperature reaches a certain point. It should be mentioned that outside the machine, near the foot of the cradle, a feeding-cup is placed, the food within which is kept warm and ready for use by means of the hot water in the tank.

MEASUREMENTS OF THE SPECIFIC GRAVITY OF THE BLOOD IN DISEASE.—Dr. Charles S. Roy gives to the Physiological Society the following method for this purpose (*The Journal of Physiology*): The abandonment of venesections has caused a neglect of this interesting and important subject, as it is no longer an easy matter to obtain from the living patient a sufficient quantity of blood to permit of its density being estimated by any of the various methods which are in use for measuring the specific gravity of fluids.

The few observations which are recorded on the density of the blood in disease show that the specific gravity of this fluid may undergo very great changes as a result of various morbid conditions. Davy, for example, gives one case in which the density of the

blood was found so low as 1038—the normal average in man being 1055.

The only instrument required for measuring the specific gravity of the blood by Dr. Roy's method is an ordinary hypodermic syringe, the needle of which has been slightly altered, its point having been taken off and the steel tube prolonged backwards so that its inner end when it is fixed on the syringe is visible from without. Besides this instrument there are required a number of solutions of salt of various densities from 1040 to 1075. Two dozen ten ounce bottles are amply sufficient, they should be wide enough in the mouth to allow of the syringe being filled from them directly.

The syringe having been half or three-quarters filled with one of the salt solutions, a portion of a drop of the blood to be examined (obtained from the finger in the ordinary way) is drawn up into the syringe, care being taken that no air is introduced with it. If, now, the blood be of greater specific gravity than the fluid in the syringe, it at once sinks to the lower part of the latter, while if it be of less density, it at once rises to the upper part of the glass tube of the syringe. In the former case, having emptied the syringe, the process is repeated with a solution of higher specific gravity, and one or two trials enable the observer to find between what two solutions the drop of blood ceases to sink and rises instead. The method is one of extreme delicacy, a very minute difference in density between the blood and the fluid employed showing itself at once by the behavior of the drop.

The difference in temperature between the blood and the standard solution into which it is drawn, might seem to have a marked influence on the result of the measurement. The effect of this is found, however, in practice to be so slight that it may, for all practical purposes, be ignored. This is doubtless due to the fact that the small part of a drop which is drawn into the syringe, at once loses a great part of its heat to the much larger quantity of fluid with which it comes in immediate contact.

The following is briefly all that is known on the subject of the normal limits of the density of blood in man:

The blood of men is somewhat heavier than that of women, the values for the former being from 1056 to 1059, while the blood of females varies from 1051 to 1055. The blood of children is specifically lighter than that of adults. It would also seem that the density of the blood is greater in the morning than it is at night, an effect apparently due to the fluids swallowed during the day.

ANATOMY AND PHYSIOLOGY.

FUNCTIONS OF THE SPLEEN.—In the last number of *Virchow's Archiv*, Professor Alex. Tauber, of Warsaw, gives as quoted by the *Lancet*, an excellent review of the theories concerning the functions of the spleen, closing with the results of his own observations.

Thus, in 1833 Tiedemann considered the spleen to be a blood-forming organ, and that after its removal its function was transferred to the thyroid body; an opinion contested by Brücke, who held there was nothing in common between the thyroid and the lymphatic system, to which the spleen belonged. Neumann and Ehrlich doubt that the spleen has any hæmapoietic function. Schiff and Herzen think that the spleen develops a ferment which is necessary to the pancreas in its digestive action, a view disproved by the fact that nutrition is not impaired by removal of the spleen. Schiff goes further by asserting that its removal does not influence, either actually or relatively, the amount of red and white corpuscles, whilst Robin, on the other hand, found in two cases of splenic extirpation in the human subject an increase in the number of leucocytes, and notable variations in the size of the corpuscles. Recently Winogradaff, of St. Petersburg, found very marked changes in the lymphatic glands, and also in the bone-marrow of animals deprived of the spleen, the former being softer than normal, and containing many free red blood-corpuscles. At the German Surgical Congress, held at Dresden in 1882, Dr. Credé showed a man forty-four years of age, from whom he had successfully removed a cystic spleen. Two months after the operation the blood was found to contain a large excess of leucocytes, but three months later their number was nearly normal. Credé had observed in the early weeks after the excision that the thyroid had enlarged in size—an enlargement which persisted to a certain extent. The lymphatic glands did not enlarge. Credé concluded from this and other cases that the splenic function was to convert white into red corpuscles, and that leucocytosis ensues on removal of the organ until the thyroid is enabled to act vicariously.

This idea was further expanded by Zesas, who published the details of experiments on the subject at the end of 1882, from which he concluded that the spleen converted white into red corpuscles, but that other organs also had this power; that increase in the number of white corpuscles and diminution in the red occur upon extirpation of the spleen until the above function is taken on by another organ, and that this property of acting vicariously to the spleen resides in the thyroid as well as in the bronchial and mesenteric glands. The lymphatic glands alone are incapable of supplying the needs of the organism in this respect, so that simultaneous removal of the spleen and thyroid is incompatible with life, this last being based on the issue of a single experiment.

Tauber remarks that practical surgery now affords more means of arriving at a conclusion, since more than fifty cases of extirpation of the spleen are on record. In 1882, previous to performing this operation for cystic degeneration of the organ, he made many experiments on the lower animals, and was thereby convinced that excision of the healthy spleen could be performed without injury to the animal's health. He practiced excisions of the spleen and of the thyroid in fifteen animals, in some removing one of these organs, in others both, and examined

the blood in each case. Six of these animals died, one from the operation itself, another from acute peritonitis; the others from weakness within three weeks of the operation, after the wounds had cicatrized; post-mortem examination showing marked congestion of the liver, lungs, and lymphatic glands, and intestinal hæmorrhages. He points out that the thyroid is very often entirely wanting in the domestic animals, and yet in such animals splenectomy is as well borne as in those having a well developed thyroid. These experiments then do not support the doctrines advanced by Tiedemann, Crede, and Zesas as to an interchange of function between these organs. Nor did Rœher in his numerous cases of thyroid excision in the human subject meet with one case in which the spleen became subsequently enlarged.

The deductions Tauber draws from these experiments, and from clinical observations, are: 1. That the spleen must be regarded as one of the main reservoirs of the blood; its removal, therefore, exerts a great influence on the circulation, as seen in the occurrence, shortly after the excision, of congestion of the liver, kidneys, and especially of the lymphatic glands. 2. No physiological affinity exists between the thyroid and the spleen. 3. An animal of middle age bears splenotomy much better than an old animal, hæmorrhages being liable to occur in the latter. 4. Animals deprived of the spleen can bear living young, in whom the spleen is present. 5. The removal of the spleen does not impair the digestive function or nutrition. 6. After removal of the spleen the animal becomes anæmic; the relative and positive number of white corpuscles notably increases, whilst the size and number of the red corpuscles diminish.

SURGERY.

AN EASY AND SAFE METHOD OF SOUNDING FOR IMPACTED GALL-STONES.—With this title as its heading, Dr. George Harley communicated a paper to the Royal Medical and Chirurgical Society (*Medical Times*) which contained the details of the case of a lady 36 years of age, who had been for many weeks suffering from the signs and symptoms of obstructed bile duct. After placing her under an anæsthetic, Dr. Harley inserted a six-inch long French exploring trocar midway between the umbilicus and margin of the liver, an inch and a half to the right of the median line. Its point being pushed upwards and backwards in the direction of the common bile duct, no hard substance was met with; on the stilette being withdrawn ascitic fluid came away, and the canula could be moved freely in all directions. The instrument was withdrawn and re-inserted an inch higher up, and about two inches to the right of the umbilicus. On pushing it in the same direction as before, to the depth of six inches, its point struck into a hard substance, presumed to be a biliary calculus. It was endeavored to estimate the size of the stone by pressing the end of the canula without the stilette firmly against the hard substance, and moving the point of the instrument all round it. The inference was that the stone was the size of a hazel-nut. The punctures were closed by means of sticking-plaster, and the abdomen bandaged. The signs of obstruction began rapidly to disappear, and it was supposed that the operation had caused the stone to change its position in the duct, and had thus enabled it to pass along into the duodenum. Convalescence at once set in, but was of short duration, for an attack of enteritis supervened, followed by peritonitis, and the patient succumbed twenty-seven days after the sounding, and twenty-four after the stone had evidently left the duct. At the autopsy, the thirteen calculi shown to the Society were found still in the gall-bladder, the longest being an inch in length, the next the size of a hazel-nut, the remainder all much smaller. The facets on the opposite ends of the large calculus showed that another stone, the size of a hazel-nut, must have existed, but come away. The paper ended with the following conclusions:

(*a*). That the presence of an impacted gall-stone may be readily, as well as safely, ascertained in the way described.

(*b*). That not only the position, but even the size and shape of an impacted biliary calculus may be instrumentally ascertained.

(*c*). That a knowledge of these facts may possibly induce surgeons to undertake the earlier artificial removal of dangerously impacted gall-stones than heretofore; an operation which ought to be, under ordinary circumstances, no more hazardous to the life of the patient than the operation of lithotomy. He believed that the fatality that had hitherto attended the operation had been almost entirely due to the fact of its having been delayed until the exhaustion of the patient precluded the possibility of recovery.

In the discussion which followed the reading of this paper, the members present seemed to think the operation anything but an easy or safe one, judging from the views expressed by Sir William MacCormac, Dr. Dyce Duckworth, Dr. O'Connor, and Dr. C. J. Hare.

THERAPEUTICS AND MATERIA MEDICA.

MENTHOL.—Mr. Archibald D. Macdonald, in a note to the *British Medical Journal*, calls attention to the fact that he was the first to point out the antiseptic and anti-neuralgic properties of menthol, in the *Lancet*, in 1879. He uses this drug in tinea tonsurans with success, both simply in rectified spirit, and in a pommade with iodoform and vaseline.

In facial neuralgia, some forms of sciatica, neuralgic headache (clams), and in toothache, he has repeatedly given relief by its use, both in the form of the stick or cone, and with the following formula—menthol, 30 grains; spirit of rosemary and rectified spirit, each 2 drachms.

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BLINDNESS FROM OPHTHALMIA NEONATORUM AND ITS PROPHYLAXIS.—The possibility of blindness occurring to a human being from any cause whatever, and at any period of life, is of such vital importance to everyone concerned that it is not possible to give too much attention to avert such a possible danger. This is especially the case where any neglect of the helpless infant, when ushered into the world, may involve the absolute deprivation of the sense of light for a whole life-time. That many have been in the past, guilty of such neglect, is manifest from the testimony afforded by the statistics of the victims to a life-long blindness. Magnus,[1] of Breslau, gave as his observation up to the time referred to, that 34 per cent. of the blind in the institution at Breslau became blind as a result of ophthalmia neonatorum. He has, however, in his recently published work Die Blindheit, ihre Entstehung und ihre Verhütung, reduced, by further observation, the percentage to 13.25 per cent., whilst the percentage of the aggregate of a number of observers in different parts of the German Empire amounts to 10,876 per cent. Calling this number for the sake of convenience 10, we have the astonishing statement that 10 per cent. of all the blind, that is totally blind, in Germany, have been thus deprived of sight through ophthalmia neonatorum. This it must be remembered takes no cognizance of those cases where the sight has been only more or less impaired. We have not at hand the necessary statistics to enable us to form an opinion as to whether an equally alarming result would be furnished by our own institutions; but from the figures at our disposal it is probable that an equal number of victims which swell the ranks of the blind in our own country are from the same cause deprived of sight. And they are consequently not only practically deprived of contributing to the world's wealth, but dependent on the contributions of others. It is well, in order to form an adequate conception of what these figures mean, to refer to the figures of Dr. Appia as quoted by Magnus. He states that France spends nine million francs a year for the support of the blind, and if 10 per cent. of this is caused by ophthalmia neonatorum, we have an expense of nine hundred thousand francs for the sustenance of those suffering from this affection alone. According to the calculation of Magnus, the whole cost to Prussia of its blind is twenty million marks a year, and making a similar allowance of 10 per cent. of ophthalmia neonatorum, we have an annual sum of two million marks entailed on the government by this affection alone.

We have only referred to this economical aspect of the question, however, in order to bring into relief the magnitude of the question under consideration. No physician needs such a reminder to induce him to discharge his duty. If the ambition of doing all that he can do for a given case and doing it well, is not an inducement strong enough, no question of economy will supply the necessary zeal. As a matter of fact this 10 per cent. of blind from ophthalmia neonatorum, as shown by the observations of Credé at Leipzig, is simply the result of negligence or ignorance which, by due care, can be completely avoided. We cannot by this be understood as saying that no case of ophthalmia neonatorum needs be followed by the loss of an eye, but we do say that no case should be allowed to get into that condition where such a result is unavoidable. Care should be taken as early as possible after delivery to have the child's eyes washed with clean water and a little soft linen or a wad of absorbent cotton, each piece when it has been used being thrown away to give place to a piece unsoiled. Should any suspicious accumulation be seen, however, in the region of the eyes it should be carefully wiped off before the process of washing takes place. It should not be necessary to remind those who undertake such a duty that whatever there is objectionable in the parturient canal will be sure to find a lodgement in the angles of the eyes, and consequently no one can claim to have given cleanliness a fair show who has not given special attention to these regions. Successful efforts have been directed by Professor Credé,

[1] *Breslauer Arzliche Zeitschrift*, 1882, No. 19.

of Leipzig, to the prophylaxis of this affection and the admirable results which he has obtained, convinces one that if the 10 or 11 per cent. of the blind attributable to ophthalmia neonatorum, continue to figure in our statistics of the blind, it will be because the little ones have been ignorantly or negligently attended to.

The records[1] of the Leipzig lying-in hospital from the beginning of the year 1874 to the end of May, 1880, show that 10 per cent. of all the births there developed ophthalmia neonatorum, whilst after special attention was given to the infants' eyes, the number was reduced to one-half of one per cent., in fact an individual case, which it was found had not received proper attention. The plan there adopted is such as we have referred to with the addition of a single drop of a 2 per cent. solution of nitrate of silver, which is dropped from a glass rod on to the cornea. Although we should prefer to depend on cleanliness alone, reserving the solution of nitrate of silver for the manifestation of active difficulty, yet it would appear from the observations of others that the same advantage is not obtained from simple cleanliness, so that whenever it is known that a leucorrhœal discharge exists at the time of delivery the most efficient prophylactic measures consist in supplementing the most scrupulous cleanliness with the instillation of a single drop of nitrate of silver into each eye. This seems the more plausible, as Professor Credé assures us that no evil consequences whatever associated with its application. Children born at full term exhibit merely a slight redness, never any swelling, whilst in children of premature birth a slight increase of secretion is developed, which disappears at the latest at the end of forty-eight hours. He claims that after the instillation of the nitrate of silver no further attention is necessary.

The probability is, however, that the greater part of the cases of ophthalmia neonatorum occur in the hands of midwives, consequently any prophylactic measures should be of such a nature that the midwives can readily be trusted with them. Now a 2 per cent. or a 10 grain to the ounce, solution of nitrate of silver, can scarcely beentrusted to the average midwife for instillation into the eye of an infant, consequently we should prefer to trust to a scrupulous cleanliness early effected, and consider the question of nitrate of silver later on if the disease displays itself. This position is justified by the teaching of both ophthalmologists and by those who devote their attention especially to gonorrhœa. Both specialists tell us that active agents in the early stage of the disease are to be avoided.

[1] Magnus, die Blindheit, etc., p. 300.

PROGRESS OF CHOLERA IN FRANCE.—The chances of controlling the epidemic within the cities of Toulon and Marseilles are by no means as hopeful as they were a week ago. The number of cases has steadily increased in those cities, and the disease is appearing at other points. Quoting from the dispatches of this morning, July 8, we find that night before last there were sixteen fatal cases in Marseilles, and during yesterday up to five o'clock there were twelve.

At Toulon during the same night there were nine deaths, and during yesterday there were seven.

Two cases were reported yesterday at Nice. Eighteen hundred persons left Marseilles yesterday.

An English steamer from Marseilles was signalled off Lisbon July 6, having two cases of cholera on board.

Two refugees from Toulon were found at Saluzzo the same day, victims of cholera.

Dr. Koch, of the German Cholera Commission, is busy at Toulon making examinations of cholera patients. He states that the epidemic is Asiatic, and finds *microbes* of the same class as in Egypt and India.

He predicts a general spreading of the disease from so central a point as Toulon.

Such are the commercial relations of Great Britain with France that it seems well nigh impossible to establish the absolute quarantine which was established in Egypt last year, and we may look for the appearance of the disease along the great centers of travel, as a matter beyond control.

We again urge all who may in any way be responsible for the sanitation of the highways of travel and the health of cities to leave no effort untried which shall promise restraint to the spread of the epidemic or which may lessen its fatality.

The popular germ theories and associate doctrines of contagiousness, greatly exaggerated by the newspaper press, are adding to the terror of all classes of people, and will correspondingly increase the destructive effects of the epidemic wherever it makes its appearance. For of all the predisposing causes of cholera, fear, dread, and mental trepidation are among the most efficient. While cleanliness of person and premises, wholesomeness of diet, and a fearless contented mind, usefully occupied, will do more to protect the individual in the midst of a cholera epidemic than all other prophylactics put together.

CHOLERA GERMS AND CHOLERA VIRUS.—It is well known that Prof. Koch, in his communications to the

German Government, claims as the result of the investigations of his commission, both in Egypt and India, the discovery of a bacillus which is specifically essential to cholera, and readily found in every case, in the mucous membrane of the intestines and in the rice-water stools. It is also stated that the same variety of bacillus was found in some of the water used by the natives in India.

Koch states that these bacilli are peculiar as to shape, motion, and tendency to aggregation.

He does not believe them to be simply concomitants of disease, or normal intestinal inhabitants.

They are not found in the stomach or matters vomited, which were not obviously of fæculent character.

He has thus far failed to inoculate animals with these bacilli. They may be developed rapidly outside the human body when subject to warmth and moisture. They are readily destroyed by drying, and by contact with fluids even slightly acid. Alkaline solutions are not thus destructive. A field for further investigation is here opened, which, whether confirmatory or corrective of the views of Prof. Koch, can hardly be over-estimated in importance and its wide-reaching relations.

But while Koch has failed to propagate the cholera in any of the lower animals, either by inoculating them with his cultivated bacilli, or feeding them with the intestines of cholera patients, Mr. Vincent Richards, of India, has been more successful in his experiments on pigs, as the following extract from the *London Lancet* of May 10 will show:

"The *Indian Medical Gazette*, for April, contains a paper by Mr. Vincent Richards, detailing his experiments on pigs, which, as stated last week, were at first erroneously thought to be confirmatory of the opinion of Koch, that a bacillus is the essential agent in propagating the disease. Mr. Richards, who had previously failed in obtaining results in the dog, tells how he was led to administer choleraic discharge to pigs. A young animal so treated was attacked with vomiting, and died in a few hours. Another pig was fed on the contents of the intestines of a pig killed by the choleraic discharge, and remained unaffected; it also was not affected by the alvine discharge from a patient in the first stage of cholera, but was subsequently killed in eighty minutes by the administration of an undoubted choleraic evacuation. Decomposing choleraic discharges had no effect on the pigs; but, as in other instances, the administration of recently-passed stools proved more or less rapidly fatal, with symptoms of asphyxia. Mr. Richards concludes that the alvine discharges at certain stages of cholera contain a powerful poison, whose chief action is to enfeeble and destroy the function of respiration. The rapidity of its action excludes it from being an organism, and he thinks that it is a chemical compound, probably of albuminoid nature. It remains for chemistry to isolate the body, and for experiment to determine accurately its physiological action; but Mr. Richards thinks that it will probably be found to be easy to disinfect the evacuations by permanganate of potash. He admits that, although the poison decomposes, it might, by desiccation, retain its powers for some considerable time, so that clothes, etc., stained with choleraic discharges might be a source of danger."

From the above statement it is apparent that Dr. Vincent Richards conceives that the poison of cholera is of a *chemical* character, and his experiments were instituted with the view of isolating this virus, and communicating it to animals successively, but not, like Koch, searching for a cholera bacillus.

If epidemic cholera is essentially dependent for its existence and propagation, upon a peculiar bacillus, which can be easily destroyed by weak acid liquids, as stated by Koch, it would seem as though it might be easily cured if treated by acids in the early stage of the attack, and its spread more effectually prevented by disinfecting the discharges with acid disinfectants, than by armed sanitary cordons. Yet both have been tried many times, in epidemics of former years, with results by no means uniformly successful.

BRITISH MEDICAL ASSOCIATION.—The fifty-second Annual Meeting of this Association, will be held in Belfast, on Tuesday, Wednesday, Thursday and Friday, July 29 to August 1, 1884. Local Secretary, John Moore, M.D., 2 Carlisle Terrace, Belfast.

SOCIETY PROCEEDINGS.

WARNING TO THE MEDICAL PRACTITIONERS IN REGARD TO THE USE OF JEQUIRITY.

BY M. LANDESBERG, M.D.

Read before the Philadelphia County Medical Society, May 21, 1884.

The medical journals have not failed to inform their readers that a new remedy has been introduced by Wecker into the oculistic therapeutics, which, by its prompt, energetic and sure action in trachoma and pannus, by far surpasses all the other methods of treatment ordinarily used in this affection. These glad tidings have been corroborated by the casual publication of notes and comments on the many excellent results obtained by Wecker and his followers by the new procedure, which consists of applying an infusion of jequirity, of a given strength, to the surface of the palpebral conjunctiva, in order to produce

rapid suppuration, and by means of the latter to promote absorption of the trachomatous infiltration. The method of treatment is based upon the same idea which led to inoculation of blennorrhæic pus in cases of pannus. I have not seen any medical paper at my command dwelling upon the great dangers in which the diseased eye is apt to be involved by the process of suppuration, and by the possible excess of reaction. I have not seen pointed out the fatal consequences which may develop in some instances. The glittering side of the question has only been made conspicuous, and it has not been considered worth while to show also the reverse of the medal. That this sin of omission may prove a source of trouble and mischief to some medical practitioners is obvious. The general physician is not in the position to follow all the intricacies of the experiments with the new drug. He relies for information on his medic al journal, and the latter tells him of jequirity as the panacea in trachoma and pannus. Now suppose he just has such a case under hand, which had proved rebellious to the treatment with the usual remedies. He finds jequirity highly spoken of in his journal, and he avails himself of the opportunity to win by easy means the battle and the honors connected with it. And now it happens that he makes matters worse, that the very existence of the organ is endangered by the new treatment, the full recovery of which he has expected with such confidence! But I do not draw the picture from imagination; I do not speak of possibilities, but of facts that have already occurred. I relate incidents of the practical life which were communicated to me by general practitioners from different parts of our country. There was excess of reaction in some instances, and implication of the cornea in others. Besides, I have received letters in which physicians appealed to me for information concerning the action of jequirity and the expediency of using it in trachoma and pannus, urging me at the same time to lay before the profession the results of my experiments in the matter.

In answer to the many queries, by which I feel deeply honored, I have only to state briefly as follows:—

I have not had any cause to abandon my usual method of treatment in instances of trachoma and pannus, which has still given me the most satisfactory results even in the most obstinate and inveterate cases. My interest in jequirity has thus far been merely theoretical, and the successes reported from one side, and the failures brought forward by the other, have only served to uphold my position of objective observation. The question is by no means ripe for verdict. Only the future can show whether jequirity will gain a permanent place in the oculistic therapeutics, or will share the fate of the many other "new remedies," to sink into well-merited oblivion after a short period of dubious fame.

But if I cannot produce the results of my own trials with the use of jequirity in trachoma and pannus, I am able to give my experiences on the action of jequirity, which I have gained from experiments made by others. The issue of these experiments in nine cases, which came under my observation, indicates plainly enough the course the general practitioner has to take in regard to jequirity.

Of the nine cases above mentioned, the result of the treatment with jequirity was negative in five cases of trachoma and pannus. There was no improvement whatever, but no injury done either. Two cases presented with trachoma and pannus, deep corneal ulcerations, which were asserted to have developed during the treatment with jequirity. Before the latter had been started, the patient had enjoyed fair vision. One patient, with xerophthalmus of both eyes, suffered the loss of the right eye in consequence of panophthalmitis, which had set in on the fourth day after the application of jequirity. There was not the slightest improvement in the left eye. A girl who had done nothing for her eyes up to the time of the treatment with jequirity, presented herself six weeks later with the following conditions: Lids thickened; palpebral conjunctiva intensely swollen, covered thickly with large granulations, and furrowed with tendinous cicatrices. Both corneæ opaque and vascular.

GYNÆCOLOGICAL SOCIETY OF BOSTON.

Stated meetings, Nov. and Dec., 1883. Wm. G. Wheeler, M.D., President, in the Chair; W. O. Hunt, M.D., Secretary *pro tem.*

(Nov.) A. P. Weeks, M.D., gave a detailed account of a case of *Acute Inversion of the Uterus*, which had occurred in his practice, followed by an essay, statistical and otherwise, upon the same subject.

Mrs. M., aet. 22, primipara, gave birth to a child at full term, in which, however, on account of ineffectual pains, the assistance of the forceps was required. A competent nurse rendered efficient help. After the birth, it was observed that the ordinary globular tumor, which the evacuated uterus forms in the hypogastrium, was becoming less firm and distinct, a digital examination discovered the placenta in the vagina; upon this, a brisk hæmorrhage set in. Renewed investigations made it evident that the uterus had inverted. The whole hand was at once carried into the vagina, grasping and compressing the fundus, meanwhile that the left hand supported the abdomen. Thus reduction was completely accomplished and with but little loss of time. The hand was retained in the cavity and ergot and ice employed until firm contraction took place. Patient made a rapid recovery.

The reader considered this a typical case, unattended with the usual complications of a rapid delivery, a short funis, traction of the cord, irregular compression of the fundus, or undue bearing-down efforts on the part of the patient, conditions which are regarded as the ordinary factors of the accident.

A condensed report of 25 cases followed; 6 occurred in primiparæ, 8 in multiparæ, others unknown; a physician was in attendance in 14, midwife in 7; traction of the funis in 6 cases; compression of abdomen, slight, in 1; no force applied as factor in 14; placenta adherent in 14; non-adherent

in 11; os contracted in one, dilated in 8; uterus firm in 6, flabby in 5; inversion reduced in 21, non-reduced in 4; recovered, 20; died, 5; one case occurred at 2 months, two at 4, and two at 6 months. A careful analysis shows that inversion is about equally liable to occur in primiparæ and in multiparæ; that the strong are victims of the accident as well as the weak; that it both complicates rapid deliveries and long, tedious labors; that it occurs alike in the hands of experienced physicians and of incompetent midwives; that traction and compression in delivery of the placenta, figure but slightly as determining causes; that it matters not whether the placenta is adherent or non-adherent.

Furthermore, we find that of these 25 cases, 17 occurred immediately, or within one half-hour after the birth of the child; but in only 9 of the 17 was the inversion discovered at once; as respects the former class, the interval of non-recognition varied from 2 hours to 3 days. Of the nine that were immediately recognized—of which only 4 were reduced at once—the remainder went unreduced, respectively, 4, 10, 11, 12 and 15 days, and when we say immediately, of the other four cases, we still allow a period of 1 to 7 hours. The mortality is great, being 23.05 p. c., the greater portion dying of shock, a few of hæmorrhage, and still a few of subsequent inflammation.

The treatment consists of immediate reduction; if the placenta be attached it should be separated and removed first of all, as thus the hæmorrhage is slight and the danger of mortality less.

The President, remarking upon this paper, recalled but three cases that he had seen in forty years' practice—one was his own, the others he had encountered in consultation. Considered it especially necessary to recognize the complication at the time of its occurrence. Believed the cervix fails to do its duty and, relaxing while the rest of the organ is contracting, the uterus is forced through. Did not believe the accident due either to traction on the cord or external pressure.

Dr. Marcy believed that faulty innervation might serve as an important element.

Dr. W. S. Brown, in all his experience, had never seen a case. Was surprised to learn it could occur at two months. Was always accustomed to use traction upon the cord.

Dr. Clark recalled a partial case seen with a midwife; child born, flooding, placenta attached; felt a cup-shaped depression at fundus, easily reduced. The midwife had used great external pressure and the labor had been very rapid.

Dr. Wheeler still believed his two cases due to inertia of the cervix; in the second case the fundus was wholly outside of the genitals, severe shock, no flooding. Had to keep his hand in position, after return of the organ, for two hours before he could get sufficient contraction, in the meantime using ice, ergot, alcohol, etc.

Dr. M. L. Brown had had a case within the past week. Child born before he entered the house, and husband said all was right, but he found the patient cold, nearly pulseless, forehead wet and cold—in a word, in a state of collapse. Upon vaginal examination, found cervix relaxed and fundus presenting in vagina, its projecting surface being firm and of wrinkled feel; reduction easily accomplished; no further trouble.

Dr. Church had often encountered inversion in the lower animals, in the mare, cow and sow; considered the cause an imperfect "make-up" of the animal.

Dr. R. L. Loring, corresponding member, agreed with previous speaker that traction was not a cause; Karl Braun uses traction and pressure; Mathews Duncan traces the accident to relaxation of the fundus which, being crowded down, is seized by the cervix like a child's head and extruded. Himself was disposed to look further and would suggest a possible unequal distribution of force in the uterine muscles, resulting in abnormal expulsive power.

Dr. Weeks responded and closed the discussion. Playfair imputes the fault to the accoucheur, while *per cont.* Sir Jas. Y. Simpson believed the condition beyond his control. Emmet attributes to too rapid delivery, to traction, pressure, assuming too soon the upright position. Lusk is positive that rapid delivery has but little to do with it.

Society adjourned.

Stated meeting, 2nd Thursday, December. President Wheeler in the chair, Henry M. Field, M.D., Secretary.

The chair announced the death of Dr. Marion Sims, of New York, an early and highly honored honorary member, and Drs. Warner, Marcy and Field were appointed a committee to prepare a proper memorial and present at the next meeting.

Henry A. Martin, M.D., read by appointment a paper on Listerism.

He began with a distinction between the germ theory and antiseptic surgery thereupon based, and Listerism. In the former he had his degree of confidence; the latter he regarded as one of the latest prevailing medical fashions and he had little respect for it.

Listerism, in its inception, had performed its service for the profession and the world—this he would not under-rate. It had converted many a former "murder-pen" of Europe into the modern cleanly and well-appointed hospital; it had substituted cleanliness and purity for dirt. On the other hand, it requires too frequent interference with the wound; and this is a grave fault in its method.

But, calling attention to what was before very generally known and only waiting to be put into operation, would never make a man famous. To bring fame it needed that a peculiar and largely absurd theory should be superadded; and it is this theory which chiefly constitutes Listerism. That Prof. Lister is earnest, and true, and honest, he did not doubt for a moment; but he did not see the inconsistencies which were presented to other minds which did not embrace his system; Hahnemann could never see anything calculated to upset his theories.

It has been proved that at a certain stage of animal decomposition a poison is developed, virulent and septic, which at a later stage seems to decompose

or consume itself and become innocuous. The reader showed at length and with force that this observation could not be made consistent with Listerian theories.

The former method with perforating wounds of the chest was to seal hermetically either opening; and the patient died under the treatment. Now that the wound is left open at both ends, kept clean, and effective drainage provided, the patient has a chance of recovery; no germicides are needed.

According to Prof. Lister, wounds ought to do very badly in a malarial atmosphere where the air must literally pullulate with bacteriæ; but, as matter of fact, wounds thus circumstanced do as well as elsewhere.

Again, observation of the relation of many of the so-called nuisances of the present day, both to public health and especially to the health of those who are directly engaged, *i. e.*, the employés, does not support Prof. Lister's theories. A certain suburb of Paris is devoted to the preparation of sewerage and excrementitious matters for agricultural purposes—such material being exposed to heat and other cicatrizing influences. The resulting smell is constant, intense, horrible; but wounds contracted in this pursuit do better, as a rule, than they do in Paris. Cholera did not invade this region at all upon occasions of late epidemics. A man named Ward set up a somewhat similar business some years ago in a portion of Roxbury; he was repeatedly complained of, most disastrous effects to the public health were freely predicted, and such apprehensions shared by almost everyone, physicians included; but he had hold of a good thing, was making money rapidly, and successfully resisted all efforts to disengage him—chiefly, however, because it could never be proved that the business had anything unhealthy about it. So with soap-boiling factories, where highly rancid fats are subjected to heat; the air does not appear to be vitiated so far as public health is concerned, and wounds contracted, by those engaged in the work, heal as well and as rapidly as under more pleasant circumstances.

In erysipelas, there is said to be a special or specific bacillus; but this Dr. M. very much doubted, because he had been able so often to cure the disease by means of no other agency than simply the rubber-bandage. A distinction should be made between erysipelas and simple erythema; the latter is the commonest occurrence after vaccination, and too often is diagnosticated as erysipelas. A pathognomonic condition of the latter is the presence of vesicles or blots; an erythematous eruption or blush, unaccompanied by this characteristic symptom, cannot properly be called erysipelas.

Dr. Martin was occupied in his argument close up to the hour of adjournment, the main features of which have been given in this abstract.

Society adjourned.

CHICAGO MEDICAL SOCIETY.

Regular meeting, held June 16, 1884.

The following paper, essay, and reports of cases were presented: On the Significance of Jaundice in Diagnosis, by Dr. Wm. E. Quine. The Effect of Noises Upon Certain Forms of Deafness, Dr. G. F. Hawley. A Case of Genito-Urinary Surgery and Median Lithotomy, Dr. Wm. L. Axford. Pathological Specimen of Papillomatous Ovarian Cystic Tumors, Dr. Charles T. Parkes.

The first paper, on the "Significance of Jaundice in Diagnosis," by Dr. Wm. E. Quine, of which the following is a brief synopsis:

After alluding to jaundice of obstructive and non-obstructive origin, and to the various theories that have been advanced in explanation of the latter, the writer succinctly pointed out that the attendant symptoms were mostly due to the presence of colorless bile acids, the bile pigment being harmless. Cerebral disturbance due to hepatic derangement, and associated with cholæmia, was not caused by the presence of normal bile, but by a different though unexplained species of toxæmia, although slight jaundice is often overlooked, and in some instances cannot be distinguished from cachexiæ, except by the discovery of bile acids in the urine. The diagnosis usually relates to the discovery of the cause, and not to the mere recognition of the effect.

1. Jaundice occurring suddenly, in apparent health, and painlessly, is usually of emotional origin, and transitory.

2. When it depends on disease or injury of the brain, acute atrophy of the liver, snake poison, or an infectious fever, it is always associated with mental disturbance.

3. If it be attended with fever, and well marked, it is secondary to inflammation of biliary passages, pneumonia, toxæmia, or infective inflammation of the portal vein.

4. If it occur suddenly, and is preceded by paroxysmal pain and vomiting, it is caused, nine times out of ten, by biliary calculi.

5. If it be preceded by typical symptoms of gastro-duodenal inflammation, it is obviously of catarrhal origin.

6. Impassable obstruction of the common duct is shown by great intensity of jaundice, clay-colored stools, and in recent cases by distension of the gall-bladder.

7. Jaundice caused by sudden obstruction of the biliary passages is always associated with paroxysmal pain and nausea, but there is no means of ascertaining the nature of the obstructing body, except its discovery in the stools.

8. In the rare cases of sudden obstruction by cancerous, hydatid and aneurismal tumors, there is almost always a history of impaired health, enlargement and deformity of the liver, ascites, etc., which, aided by the revelations of physical exploration, will lead to correct differentiation.

9. Sudden return of normal coloring to fæces confirms the diagnosis of obstruction.

10. Occlusion of the cystic duct may be attended with as much pain, nausea, and distension of the gall-bladder, as occlusion of the common duct, but there is no jaundice. In occlusion of the hepatic duct, the same symptoms are present, including jaundice, and excluding distension of the gall-bladder. It is often

impossible to distinguish between occlusion of the hepatic and the common duct. The former is rare because the duct increases in size from above downward.

11. If jaundice persist after the symptoms of biliary colic or catarrhal inflammation have a month since disappeared; or if jaundice have disappeared after a biliary colic, to return slowly and painlessly, it may be assumed that stricture of the duct has resulted from inflammatory thickening, adhesion of walls, or cicatrization of an ulcer.

12. A history of repeated attacks points to the probability of gall stones.

13. If jaundice come on slowly, without antecedent colic, or catarrh, and without attendant evidence of impaired health or portal obstruction, it is probably caused either by pressure upon the duct, or by the growth of a tumor within its walls. The pressing body, when large enough, may be readily appreciated, as in the case of pregnancy, ovarian tumor, aneurism, distended colon, etc., but when it is small, or constituted by enlargement of lymphatics in the fissure of the liver, it is apt to escape detection.

14. Slight but persistent jaundice may be due to incomplete occlusion of the common duct, or to complete occlusion of a branch of the hepatic; but usually it is found associated with either valvular disease of the heart, some disease of the lungs which obstructs circulation, or cirrhosis of the liver.

15. If ascites be associated with it, the disease is either cirrhosis or cancer of the liver. If the liver be abnormally small, the disease is cirrhosis; if it be large, the disease is either hypertrophic cirrhosis or cancer. Differentiation between the two is seldom attended with difficulty.

16. Absence of jaundice does not imply absence of hepatic disease, since the liver may be destroyed by disease, or extirpated by operation, without jaundice ensuing.

17. It is not a prominent symptom of hepatitis if catarrhal inflammation of biliary passages be rigidly excluded. It is not characteristic of hepatic abscess, where, at most, mere muddiness of the complexion is usually seen. These affections are rare in temperate climates, and when encountered, are generally found to be secondary to direct injury of the liver, or to infective inflammation of the portal vein. It is not a symptom of waxy or fatty liver, or of hydatids, excepting as an extraordinary complication.

DISCUSSION.

Dr. G. C. Paoli said, there is no doubt but in many serious derangements of the liver, few or no symptoms of jaundice are present. If so, they are passed unnoticed. In a clinical point of view, the paper is a most valuable one, and others should be written and cases presented. In scirrhus of the liver, oftentimes, there may be no pain and but light pigment deposit, in advanced stages of the disease. In other maladies, too, there will probably be but very little icterus, and no bilious excretions passed from the bowels. In a chronic state of the liver, where there is no organic trouble, we may pass by the yellow appearance of a conjunctiva, as, being a symptom of liver difficulty, and during the early stages of cirrhosis or cancer of this organ, we will probably not diagnose it.

Dr. S. H. Stevenson asked the writer to explain more particularly regarding cholesterine, and obstruction of the gall duct, if these conditions are toxic in effect, or if they produce cerebral disturbances.

Dr. J. J. M. Angear, who argued at some length, stated, that if the odor and clay-colored stools came from the large intestine, and this had nothing whatever to do with the bile or small intestine, then he could not understand why the liver excreted bile. To give mercury produces dark-colored, offensive stools, because why? it affects the large intestine and liver. Physiology tells us bile destroys the red corpuscles of the blood. If the coloring matter of bile, then, is nothing more than the coloring matter of the blood, then why not take the position, that, inasmuch, if bile be toxic, that some poison has entered the blood, and that the icteric condition of a conjunctiva and mucous membranes has nothing to do with the liver? why not, I say again, take the hypothesis that there has already existed a blood-poisoning? and closed by complimenting the writer on the many good points contained in his paper.

Dr. J. H. Etheridge inquired, if people at times who are laboring under mental emotion do not become greatly jaundiced? Is it not a fact that pigmentary substances contain more than one kind of coloring matter? Regarding those who are hurt or injured, when they become jaundiced, is it due to a vascular condition, or to reaction? or is the icterus due to a nervous condition or to shock of the nervous system, or to a change resulting from a condition of the vascular merging into a nervous system?

Dr. C. T. Fenn stated, in cases of hepatitis, that result fatally, a condition just after death resembling icterus will come on, due to the cause of death, namely, abscess of the liver. If this be so, he queried the reason of it?

Dr. R. Tilley and others debated the subject, and Dr. Quine closed, but would not attempt to answer all the questions, nor could he inform the members where to seek for the necessary information. But it has not been proven that bile is toxic, nor that cerebral disturbances result from it. The blood and secretions may be saturated for years, and no cerebral disturbance follow. And in cases of obstruction of the common duct, no phenomena of cerebral disturbance need develop. Regarding the secretory functions of the liver and feculent discharges, the theory advanced by one speaker is not sustained by the every day practitioner, although some remedies do increase the secretory action of the liver. Regarding jaundice not appearing in scirrhus, he thought this was erroneous. The paper did not dwell upon the conditions that are present in catarrhal jaundice, biliary calculi, nor congestion of the liver. The condition of insanity or emotional mental trouble is differentiated by the history of the case. Regarding Dr. Fenn's interrogatory, he did not know that jaundice occurred after death.

"The Effect of Noises upon Certain Forms of Deafness," by Dr. G. F. Hawley.

More than two hundred years ago Dr. Thos. Willis, of Amsterdam, described certain forms of deafness in which hearing was improved by noise, enabling persons to distinguish tones and sentences which at other times would be unheard. This symptom, bearing the name of Paracussio Willisiana, has been accepted by most aurists, but no satisfactory explanation as to its cause has been advanced.

While Politzer, Von Trollsch, and others, consider that it depends upon some change in the position of the ossicles or membrana tympani, many like Kramer take different views, and believe that it is due to a stimulating of the auditory nerve, thus forcing it to a more healthy action. Experience favors the theory that the middle ear is the seat of this symptom, that some change is brought about by the constant noise which restores the hearing for the time being.

What this change is, is still open to discussion. It may be of interest to relate the following case which came under the writer's observation while house-surgeon at the Golden Square Hospital, London. Mrs. H., aged 30, had for the last five years suffered with marked deafness in both ears. Common conversation was heard with difficulty except when any loud noise was present. At that time the hearing apparently was greatly improved. A watch could be heard on the left side only upon contact, and on the right at a distance of about two inches. The tuning fork showed no trouble of the acoustic nerve, but clearly indicated some disturbance of the middle ear. The drum-head was normal as to tension, color, etc. While perfect movability existed in the mallio-incudal joint. Nothing indicated anchylosis of any of the ossicles. The appearance of the throat was healthy and the eustachian tubes both patent. This is a good example of this form of deafness in which the symptom of hearing better in a noise manifests itself. Roosa, however, mentions two cases where the drum-head was more or less destroyed and still noises improved the hearing. The ossicles in both cases were intact. Politzer, in his work on Aural Surgery, states that this improvement is seen only in middle ear affections of an adhesive or sclerotic character, and regards it as a most unfavorable symptom for satisfactory prognosis. Roosa, on the contrary, states that it is found in many forms of ear affections, and does not necessarily forbid one from expecting favorable results from treatment. In his article on "The effects of noise upon diseased and healthy ears," he says, I have known two cases where this symptom occurred in patients who regained their hearing perfectly. While the symptom frequently accompanies incurable diseases of the middle ear, I believe it is a very frequent symptom in sub-acute cases where both ears are affected. Of course it would not be observed in disease of one ear only. Whatever may be the cause of this symptom, we are justified, the writer thinks, in believing that certain pathological changes are then present in the middle ear which will be found at no other time. If this be true, in what does the change exist? Willis considered the change brought about by the action of strong wave tone upon a relaxed membrana tympani to be the cause of this symptom. As long as the drum-head is in a relaxed condition it refuses to respond fully to wave impressions. It is no longer a perfect sound-conductor until its tension is restored. Noises by forcing air against the membrane press it inward, restore its tension, and hearing is improved until the pressure is removed, when deafness returns as bad as ever.

If this improvement were found only in such cases as he describes, we might accept his theory as correct, but unfortunately, as already stated by the writer of the paper, it is present with no change whatsoever in the drum-head. Therefore, in such cases at least, this improvement in hearing is due to some other cause. We have in the middle ear two other elastic membranes, one of which takes as important a part in conveying wave impressions as the membrana tympani. Any change by disease or injury would be fully as disastrous to normal hearing, *i. e.*: By this the writer means the membrane of the foramen ovale. When we consider the extraordinary fine shades of vibrations transmitted by this membrane to the fluids of the labyrinth, are we not justified in expecting that any loss of accuracy and precision on the part of the transmitting apparatus at this point will cause a corresponding loss of hearing power? According to Riemann the excursions of the stapes in the fainter tones are so small as to escape detection even with the highest power of the microscope. Therefore a corresponding sensitiveness on the part of the membrane must necessarily be present in order to appreciate and transmit these vibrations. Thus we see that any change in the membrana ovale which tends to hinder the passage of the vibrations to the fluids in the inner ear must cause diminution in hearing. If then the membrane from any cause becomes relaxed, and loses the power of responding to vibrations, it acts no longer as a sound conductor, but as a damper to any wave impressions. This is demonstrated in the case of relaxed drum-heads where inflation improves the hearing. Not only must this elastic plate (so-called by Politzer) be capable of responding fully to any wave impulse, but it must also bear a certain relation to the foot of the stapes, *i. e.*, a due amount of pressure must be applied by the stirrup and a sufficient resistance be afforded in every ear that possesses perfect hearing. If this is lost, deafness is the result. Chronic suppurative inflammation of the middle ear with destruction of drum-head, incus and malleus is a proof of the correctness of this statement. Hearing is here frequently improved by a false drum-head, which presses the loose stapes against the membrane of the foramen ovale, thereby restoring the relative positions of the parts. In a relaxed membrane the pressure of the stapes is wanting, due to an absence of resistance in the diseased membrane itself. Until we find some means of restoring this needed resistance, deafness remains. Thus, through the loss of elasticity in this membrane, we lose two important factors for perfect hearing. First, this membrane, like the membrana tympani, while relaxed refuses to act as a conductor of sound by not responding to vibrations given it; and, second, the proper relation between it and the stapes is destroyed, the necessary pressure and resistance being absent. We find in the

case of an atrophied drum-head that hearing is frequently improved by inflating the middle ear, which restores the tension of this membrane by forcing it outward. One might suppose that such inflation would likewise restore lost hearing if due to a relaxed membrane at the oval window. Unfortunately the act which might restore to this membrane its proper tension, forces it inwards as well ,and thus separates the stapes from it. Though the membrane may be in a proper condition for receiving and transmitting wave sounds, the stapes from the absence of any pressure applied by it to this membrane will be unable to perform its duty, there will be more or less obstruction to the passage of sound at this point. Whatever then restores the hearing in these cases must not only renew the tension of the membrane but maintain as well the relative position of health between it and the stapes. This, in the writer's opinion, can only be accomplished by some influence brought to bear upon the membrane and stapes from outside the middle ear. If now we consider the effect of wave impulses upon the membranes and ossicles, we can understand how a loud and continuous noise (as the writer thought) might improve the hearing where the deafness is due to such pathological changes as herein described. The air, set in motion by this noise, strikes upon, and pushes the drum-head ossicles inwards. This crowds the air in the middle ear against the membrana ovale, which if relaxed is made tense by this pressure. At the same time the relative position between it and the stapes as to pressure, etc., is also restored, and hearing for the time is therefore improved. The noise which would confuse with normal hearing is absorbed, so to speak, in removing the cause of deafness. In case of perforated drum-head the vibrating air acts directly upon the diseased membrane and stapes.

Von Trollsch mentions a case where a magistrate so afflicted improved his hearing by pressing a bit of stick against the drum-head. Allen also, in his work on Aural Surgery, states, while speaking of this symptom, that the hearing power was increased in a patient who presented herself with marked distruction of the drum-head, by pressing cotton against the ossicles. Another is also noted where large perforations existed. Here, before and after the healing of the perforation, hearing was improved by pressure.

Though these three cases do not in themselves place this theory beyond dispute, it may make it still worthy of further consideration.

It may encourage us to further investigations, which perchance may result in establishing beyond a doubt the cause of improved hearing in certain forms of deafness, by noise.

DISCUSSION.

Dr. S. J. Jones. This subject has been of much interest to pathologists, for different causes produce the condition alluded to in the paper, and it is not therefore always dependent on the same cause. A noise that may affect an abnormal ear will probably have no effect on the normal ear of a person. In some cases of relaxed condition of the stapes, the hearing power is increased by pressing cotton over the region of it. A tuning fork placed between the teeth in a state of vibration, is also better heard in the affected ear of some persons. Also, in placing the hand on a piano in the transmission of delicate sounds, the affected ear is oft-times excited to action by the sound.

Dr. R. Tilley did not accept the adopted theory of the essayist, although some people can hear better during the presence of loud noise. If, as is a fact, when a relaxed condition of the mucous membrane of the middle ear exists, and we inflate it, the person can hear practically well for ordinary purposes with patent eustachian tubes.

Median Lithotomy. This operation was performed by Dr. Wm. L. Axford, who verbally cited the history of the following case of genito-urinary trouble. An old man, ætat 66, of inherited gouty diathesis, occupation gardener, had been during the last year and a half a sufferer from repeated attacks of nephralgia, which could only be relieved by hypodermics of morph. sulph. gr. ¼. After several attacks this remedy seemed to have no effect in affording him relief, neither did suppositories of belladonna and other anodynes appear to have any control of the pain. He was also a sufferer from chronic cystitis. Some time since the patient was again seized with the kidney trouble and cystic, combined, which continued three days, his urine was voided in small quantities very often, as frequently as every five minutes part of the time. The bladder was full continuously, and catheterization was resorted to every six hours. This produced excessive pain, which continued in spite of large doses of opium, for four hours after. Hyperæsthesia of the urethra became very marked, and this procedure of using the catheter had to be abandoned. It was also ascertained that he had an enlarged prostate. As a last resort the operation for median lithotomy was done, which very much relieved him, and thus he was saved the torture of many catheterizations. A drainage tube was inserted, this had to be frequently removed and freed from the deposits of phosphates and retention of mucous. The urine was excessively ammoniacal and fœtid. But the patient survived only a few days. Autopsy, cirrhotic gouty liver and kidneys, cystitis of aggravated and obstinate form, enlarged prostate, etc.

The speaker thought he was warranted in making the median incision in the case he reported, and should do so again under similar circumstances.

Dr. C. T. Parkes supported Dr. Axford in what he had done in the case of cystitis—that these cases were best relieved in the manner just stated. He then briefly alluded to two cases of a similar nature, relieved by this method, that remained so for six months. Dr. Parkes then gave an interesting verbal report of a case, and also exhibited the specimen, which consisted of papillomatous multilocular cysts of both ovaries. The cysts had ruptured, and discharged their contents into the peritoneal sac. The history of enlargement embraces a period of only six months. The patient stated she never had an illness previous to the commencement of the abdominal enlargement. She has emaciated rapidly and to great

extent. The patient measured 44 inches in circumference at height of umbilicus. The greatest difficulty in the case was the differentiation between ascites and ovarian cyst. There was no œdema, nor had there ever been, of the lower extremities. Percussion gave dullness in all directions—lying, standing, or sitting, the only place of resonance being in the epigastrium. Fluctuation was very distinct on the slightest touch in all directions. Pelvic examination ascertained that fluctuation was not to be felt in the pelvis. By this was also determined the presence of a small growth about the size of the closed fist on the right side. The uterus was displaced forward, close behind the pubes; cul de sac of Douglas filled with foreign body. *Diagnosis.*—Ovarian tumor; ruptured sac; contents emptying into peritoneal sac. *Operation.*—22 quarts, or 44 pints of fluid were evacuated from the abdomen and tumors; fluid of a dark amber color. No large amount of lymph was precipitated in it after standing. Both ovaries were found diseased, there being present in the left side a ruptured papillomatous tumor. It still contained some fluid, which could not be pressed out at the site of papillary growth. The right one had not ruptured, but was bound down deeply in the pelvis by adhesions. The pelvis was filled by these tumors and their adhesions. After much trouble the tumors were enucleated and removed, which were, at this point of the speaker's remarks, shown to the members. The operation was done recently, and during the first four days the temperature of the patient had not reached 100° F. She was strong and cheerful, and she was doing well in every respect, but was not yet out of danger. Bleeding was quite free immediately after the separation of the adhesions, but soon ceased altogether under pressure with dry sponges. A drainage-tube was left in the lower end of the incision down to the bottom of the pelvis, anticipating free discharge from such an extensive raw surface. The drain was very free through it for two days. The sigmoid flexure of the colon was adherent for two inches to one of the cysts. For the past few days the temperature was normal.

Dr. A. R. Jackson remarked: The distension by the fluids, in this case, prevented the intestines from rising to the highest point in the abdomen; and it required, he thought, great skill on the part of the operator to differentiate such a case from one of ascites, hydronephrosis, or ovarian tumor, although before proceeding to operate, an examination of the fluid would have determined it to be ovarian or otherwise. Drysdale has made 2,500 examinations, and, with but one or two exceptions (the speaker thinks), has succeeded in finding the characteristic pathognomonic cell in ovarian cystic fluid. L. H. M.

DOMESTIC CORRESPONDENCE.

EFFECTS OF ALCOHOL.*

TO THE EDITOR:

Your first question:—"How does it happen that during its presence (that of alcohol), not only the products of combustion, but the sum total of all eliminations are diminished?" is premised upon a subtly devised sophism, and by the right of discovery I claim the opportunity thus afforded me—and with such an antagonist I may not soon find another—of asking, what proof can you bring forward in support of the assertion evidently conveyed by the question? Neither in lecture nor communication, have I attempted to show that the temperature of the body could be raised or lowered by the exhibition of alcohol. True, you mention that Anstie noticed a lowering of the temperature, and as you are no doubt well aware, this was in cases where complete stupor, or a condition of anæsthesia had been reached. As having a special bearing on this point, the investigations of Drs. Parkes and Wollowicz certainly occupy a front rank. From these authorities we learn that a healthy young man was given, for a period of six days, a quantity of alcohol varying from one to eight ounces daily—in diffused quantities—and subsequently twelve ounces of brandy daily, for three days, thermometric observations being made every two hours in both cases, with no perceptible decrease of body heat beyond that observed during a like period of water drinking. In support of these authorities, I might cite the statement of Rickard, who on one occasion gave as much as twelve ounces of brandy to one individual, and in "straight" drinks, without being able to note the faintest lowering of temperature.

Speaking to "the lessening of the eliminations," I know that Böcker and Hammond are of opinion that alcohol and beer tend by their use to lessen the formation of sulphuric and phosphoric acids, urea and the extractives, while diminishing the water and acidity of the urine. But, opposed to this, we find Parkes and Wollowicz declaring that neither alcohol, brandy, nor claret produce any decrease in the elimination by the urine of urea, phosphoric acid, or free acidity. That the latter investigators have the weight of evidence on their side, at least as regards acid reaction, can be easily proven by any practitioner, at the expense of a few strips of litmus. Indeed, the writer can offer nearly a thousand insurance examinations, during 1880–81, made among a class of men who in moderation or otherwise were accustomed to the use of alcohol,—as proof that their indulgence did not materially lessen acidity of the urine. In the majority of these tests only litmus was used, but in many of them a quantitative as well as qualitative analysis was made.

Your partial admission that alcohol tends to bind up the albuminous material of the body in new combinations, sufficiently answers the question referring to M. Dupre's experiments. If by its affinity for certain tissues, alcohol seeks to protect those tissues from impending destruction, for so long a period as several weeks, and then leaves them unimpaired, and itself remains unaltered, as you would have your readers infer, we cannot but praise the spirit for its evident good behavior while in the system. When we consider that the most recent investigations teach us to regard the oxidation of albumen as the necessary prelude to the formation of fibrine, and the oxi-

dation of this constituent of the muscles and blood as leading to the production of colloidal material, the importance of even the catalytic or mechanical action of alcohol is at once made manifest. In short, if disease be regarded "not as a foreign entity, which takes up its residence in our tissues, producing disturbance and destruction, by a new and foreign material, but rather as a misdirection given to our normal functions, by agents other than 'minute organisms,'" it is certain that though alcohol may not claim a place in the healthy body, it can with propriety be given a place in the diseased one. "The eucalyptus tree makes sweeter the heavy air of the Campagna. and the maggot in the putrid mass but purifies it." In like manner may not the much abused, because so little understood, alcohol serve by its mere presence to tide the tissues over the point of impending destruction by the mechanical or molecular combinations you suggest.

The concluding hint that, if pushed to a logical result, my argument would place alcohol, opium and tobacco in the same category, might be answered by quoting the Duke of Argyle, who in a recent work, tells us that: "The world is the correlation, the fulfilment and the sanction of the instincts, capacities and longings of psychic existence." Indeed, the consciousness of a reserve power in man, argues future opportunity for its adequate exercise, and though man in satisfying this longing may reach the very limit of this reserve power, as regards his appetites, and transcending the limit of proper indulgence, trespass upon the domain of license or abuse, of alcohol, opium or tobacco, such abuse is no more argument against their legitimate use, than citing the gluttonous feasts of classic or medieval days, would be an argument against the modest spread of a Chicago physician. Yours respectfully,

P. H. CRONIN, M.D.

N. W. Cor. N. Clark and Oak Sts.

*The above communication, the receipt of which immediately followed the publication in our issue of May 3, of a letter from Dr. Cronin and some queries addressed to that gentleman by the editor of this JOURNAL, has been unavoidably crowded out of our columns until now, by the unusual press of matter requiring space in the JOURNAL during and after the meeting of the Association.—ED.]

MISCELLANEOUS.

OFFICIAL NOTICE.

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RICHARD J. DUNGLISON, M.D.,
Treasurer.

Lock Box 1274, Philadelphia, Pa.

GOOD LOCATION.—I have an established practice of twenty-five years, and will retire in the favor of the purchaser of my house, furniture, office, instruments, library, barn and team.

Care of this JOURNAL. E. KNOWLTON.

OFFICIAL LIST OF CHANGES IN THE STATIONS AND DUTIES OF OFFICERS SERVING IN THE MEDICAL DEPARTMENT UNITED STATES ARMY, FROM JUNE 25, 1884, TO JULY 3, 1884.

Hartsuff, Albert, Major and Surgeon, (Fort Riley, Kansas,) granted leave of absence for one month with permission to apply for one month's extension, to take effect when his services can be spared. (Par. 6, S. O. 130, Headquarters Dept. of Mo., June 25, 1884.)

Middleton, J. V. D., Major and Surgeon, ordered to relieve Surg. B. E. Fryer from duty as Post Surgeon, Ft. Leavenworth, Kan., on or before July 1. (Par. 5, S. O. 133, Headquarters Dept. of Mo., June 25, 1884.)

Dickson, Jno. M., Captain and Assistant-Surgeon, assigned to duty as Post Surgeon, Alcatrag Island, Cal. (Par. 3, S. O. 71, Headquarters Dept. of Cal., June 19, 1884.)

Girard, A. C., Captain and Assistant-Surgeon, granted leave of absence for six months, with permission to go beyond sea. (Par. 11, S. O. 148, A. G. O, June 26, 1884.)

Girard, J. B., Capt. and Assis't-Surgeon, ordered to relieve Surg. W. E. Waters from duty as Post Surgeon, Plattsburg Bks., Plattsburg, N. Y. Surgeon Waters, upon being relieved, directed to return to his proper station, Madison Bks., N. Y. (Par. 3, S. O. 131, Headquarters Dept. of East, June 30, 1884.)

Benham, R. B., First Lieutenant and Assistant-Surgeon, from Dept. of Dakota to Dept. of Texas.

Gorgas, Wm. C., First Lieutenant and Assistant-Surgeon, from Dept. of Texas to Dept. of Dakota. (Par. 8, S O. 150, A. G. O., June, 28, 1884.)

Wales, Philip G., First Lieutenant and Assistant-Surgeon, from Old Fort Colville, Wash. Ter., to Fort Cœur d'Alene, Idaho. (Par. 3, S. O. 89, Headquarters Dept. of Columbia, June 23, 1884.)

LIST OF OFFICIAL CHANGES IN THE MEDICAL CORPS OF THE NAVY, FOR THE WEEK ENDING JULY 5, 1884.

Surgeon H. M. Wells, detached from Naval Hospital, Brooklyn, ordered to U. S. S. "Lancaster."

Dr. J. S. Sayre, Dr. F. J. B. Cordeiro, appointed Assistant-Surgeons. Commission dated 27 June, 1884.

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